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FOUR VARIETIES OF BEHAVIORISM

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With apologies, I shall present this paper[1] in the rather personal form which it first assumed in my own thinking. I am often asked by my students, and occasionally by brother psychologists, how I stand on the matter of behaviorism. Some have said to me, 'You certainly are a behaviorist, are you not?' and others, 'You certainly are not a behaviorist, are you?' It was in the effort to answer such inquiries that the present line of thought arose.

In 1899, I wrote[2] as follows:

Was that behaviorism? It certainly announced a program of objective, non-introspective investigation in psychology. Not having been followed up aggressively, it has no importance in the history of the matter. It was only rescued from oblivion by the keen eye of Titchener, who seized upon it as a recrudescence of an early and, as he judged, an unpsychological plan of attack. Titchener wrote[3] in 1905:

[1] Read at the Madison meeting of the American Psychological Association, December, 1923.

[2] 'Accuracy of Voluntary Movement,' p. 25.

"We cannot tell from introspection what guides our movements. . . . We have to rely on a quantitative determination of the degree of accuracy obtained under different conditions. Here we have a method of psychology which does not depend upon introspection. And it seems undeniable that this method ought to be applied in as many fields as possible. It has already been applied in memory and in some problems of perception. Give the 'subject' some difficult task to perform under certain conditions from which he cannot escape (much as in a game); then vary the conditions, and measure and compare the success of his efforts."

[3] 'Experimental Psychology, Quantitative, Instructor's Manual,' p. 367.

"Not a novel method, surely. Not a method whose previous application has

Titchener puts the matter in its true perspective. So far from experimental psychology having begun as a purely introspective enterprise and needing young upstarts to force it into objective channels, it made its beginning with non-introspective studies of reaction time, psychophysics, and memory, and, about 1900, through the efforts of Müller, Külpe, Titchener and others, was just beginning to take shape as a genuinely introspective science. Since that date, introspective psychology has been busy perfecting its technique, and has achieved some rather striking successes in the fields of sensation, memory, and thinking. Meanwhile the objective method in psychology, while indeed an 'old' method, has not yet been cast out as a 'bad' method, but has been fully as much used as the introspective method.

The true state of the case was best formulated by Cattell, when he said,[1] in 1904:

Was that behaviorism? So inclusive and tolerant a view does not deserve to be called an 'ism' of any sort, but rather an envisagement of the actual fact, psychology.

However, though objective observation has always bulked large in psychology, there was little excitement over it, and little attention was paid to its bearing on the general definition of psychology until Watson, a few years later, began to proclaim [2] in no uncertain tones:

been restricted to 'memory and some problems of perception.' Rather, the method of the earlier work on the 'time sense'; the method of the earlier work upon reaction time; the method which, in the domain of Fechnerian psychophysics, has led—among other lamentable results— to the distinction of Weber's and Merkel's laws. The old, bad method of arguing out a psychology on the basis of psychophysical determinations."

[1] *Popular Science Monthly*, 1904, 66, 176–186.

"There is no conflict between introspective analysis and objective experiment—on the contrary, they should and do continually coöperate. But the rather widespread notion that there is no psychology apart from introspection is refuted by the brute argument of accomplished fact. It seems to me that most of the research work that has been done by me or in my laboratory is nearly as independent of introspection as work in physics or zoology."

[2] PSYCHOL. REV., 1913; and 'Behavior,' 1914, p. 1.

"Psychology as the behaviorist views it is a purely objective experimental branch of natural science. Its theoretical goal is the prediction and control of behavior. Introspection forms no essential part of its methods, nor is the scientific value of its data dependent upon the readiness with which they lend themselves to interpretation

There can be no doubt that this is genuine behaviorism. Historically, this is where the term originated. As the outstanding fact in Watson's pronouncement is his rejection of introspection, behaviorism of this genuine and authentic sort has sometimes, in later accounts, been referred to as 'methodological behaviorism.' I do not believe, however, that such a characterization does justice to Watsonian behaviorism. As a methodology, behaviorism is almost purely negative. It stresses objective methods, which were already in active use, but at first it made no attempt to introduce new objective methods. A little later, to be sure, it proposed to introduce the conditioned reflex method; but, though the conditioned reflex has proved a very interesting phenomenon, the conditioned reflex *method*, thus far, has scarcely gained a foothold, at least in human experimentation. It appears to be more tricky and less controllable than the equally objective verbal response method, which had long been employed. Methodological behaviorism offers nothing that is characteristic on the positive side.

On the negative side, behaviorism at first seemed to reject introspection, root and branch, and its platform seemed to be summed up in one plank, 'Introspection be banned!' A little later, we note a partial relaxation from this negative attitude. Probably in justice to sense physiology, introspection was re-admitted under the guise of the 'verbal report method.' Now a verbal *response* is as objective a fact as any other response, and as open to the observation of the experimenter; but a verbal *report* means a report by the *subject* of some observation made by him, and presumably made by him because he was in a better position to make it than the experimenter. Verbal report, then, is in no way different from introspection. But behaviorism, as it had rejected introspection without any detailed critique, without any examination of its peculiar difficulties and of the precautions devised by introspectionists to meet these difficulties, so also

in terms of consciousness. The behaviorist attempts to get a unitary scheme of animal response. He recognizes no dividing line between man and brute. The behavior of man, with all of its refinement and complexity, forms only a part of his total field of investigation."

re-admitted introspection, as 'verbal report,' without any attempt to improve the method or to keep it up to its maximum reliability. In short, the behavioristic treatment of introspection has throughout been decidedly amateurish, and represents no real contribution to the science.

No, behaviorism was not, is not, essentially a methodology. It had something much more novel and characteristic to offer. Behaviorism is a *program* rather than a methodology. Taking its start among students of animal behavior, and chafing under the assumption of the introspectionists that such a study must be mere by-play in psychology, it proposed to take behavior study as fundamental. It proposed to take the animal experiment as the model for experiments on human beings. It proposed to treat animal psychology as general psychology, and to regard human psychology as a special branch. It proposed to derive its fundamental concepts from animal behavior, keeping them free of connotations derived from the more specialized human psychology, and especially free of connotations derived from introspection. This program promised plain sailing in muscular and glandular responses, and, through the medium of linguistic movements, seemed to open up into the thought processes. Sensation and perception were difficult to approach from this angle; and the behaviorists have taken the rather absurd tack of ignoring and even seeking to deny the reality of sensory and perceptual phenomena. Aside from this obvious weakness, there is something essentially sound about the behaviorist program. It seems reasonable to look for our fundamental concepts in the simplest forms of life, and on them as a basis to build up a system of secondary concepts applicable to the more complex forms.

Now there is no inherent connection between this behavioristic program and methodological objectivism. Certainly the behaviorist must stress the objective method, but he might perfectly well admit introspection in some parts of human psychology. On the other hand, one might be a thoroughgoing objectivist without taking any special interest in the behaviorist program, as we see in the case of the worker with mental tests.

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Behavior study, behavior concepts, laws of behavior, control of behavior—such is the essential program of behaviorism. But certain additional projects are sometimes put forward as parts of this program, and the question arises whether they are inherently bound up with behavior psychology in the strict sense.

Watson, from the standpoint of the student of behavior as such, has repeatedly denied that behavior psychology is identical with physiology. In studying behavior, we take the organism as a whole, or at least are not concerned with the minute analysis of its functions that is proper to physiology. In particular, the behaviorist is not concerned with the details of the nervous system; he avoids making a fetish of the nervous system.

Rather surprising, then, is the insistence by some leading behaviorists upon a neuromechanistic interpretation of all things psychological as the goal of behaviorism. Behavior study is not directly a study of neural mechanisms, behavior concepts are not directly neuromechanistic concepts. That there is no inherent connection between this type of behaviorism and the main type is clear from several facts:

1. One may be a behaviorist without attachment to the neuromechanistic project. Watson is an example.

2. One may be an introspectionist and still believe in neuromechanistic interpretation. Titchener is an example.[1] In fact, belief in a complete neural and organic mechanism for all mental life has been rather common among those who have defined psychology as the science of consciousness. They may have regarded themselves as parallelists, but we could transform them into naïve monists without changing their status as psychologists—the questions of monism, and dualism, parallelism, and interactionism being, as I conceive, wholly irrelevant in the science of psychology.

3. Lashley[2] has endeavored, with considerable success, to show that neuromechanistic interpretation can take full account of all that is well established in introspective psy-

[1] See, for example, his 'Beginner's Psychology,' 1918, pp. 17–18.

[2] PSYCHOL. REV., 1923, 30, 237–272, 329–353.

chology. That is equivalent to showing that there is no necessary antagonism between introspective psychology and this type of behaviorism, and that, therefore, there is no inherent connection between this type of behaviorism and objectivism.

4. The lack of opposition between introspection and mechanistic interpretation comes out clearly in the concrete work of Helmholtz. In his theory of hearing, Helmholtz was making a definite attempt to furnish a mechanistic interpretation of introspective data, such as overtones and difference tones. He was endeavoring to reach a conception of the mechanism of the cochlea that should square with these introspective facts. This instance is typical of the labors of sense physiology.

Thus it appears that the neuromechanistic project is not distinctively behavioristic. The same can be said of another project that, also, is highly commendable in itself. The word, 'behavior,' in common parlance, suggests socially and personally important conduct, and when behaviorists include the study of such conduct in their program, the first impression is that such 'behavior study' belongs logically in behaviorism and nowhere else. The psychoanalysts are an instance to the contrary, being concerned above all things with conduct in this sense, but not being averse to introspective methods in their study of the individual, not being impressed at all with the psychological importance of animal behavior, and not being in the least inclined to a neuromechanistic interpretation. McDougall is another instance in point.[1] There is, of course, no reason why behaviorists should not study social behavior, but on the other hand there is no reason why other psychologists should keep their hands off.

The several projects announced as belonging to the behaviorist program are not mutually inconsistent, but neither are they mutually coherent. In order to be a behaviorist, must one endorse all these projects, any one of them, or some particular one at least? Does one qualify as a behav-

[1] See his recent article, 'Purposive or Mechanical Psychology,' PSYCHOL. REV., 1923, 30, 273–288.

iorist simply by using and stressing objective methods, or must one also stress the fundamental importance of animal behavior as the source of the concepts of psychology? Is it enough to be a student of personality and social conduct, or must one be working towards a neuromechanistic interpretation?

If it is a mistake to tie up one form of behaviorism with another, it is equally unecessary to tie up behaviorism with any special hypothesis, such as that of the linguistic nature of all thought. This view of thought has been held in the past by many who could not otherwise qualify as behaviorists, and Washburn has pointed out[1] that it is the introspective evidence of inner speech during thinking, rather than any existing objective record of speech movements, that has really given rise to the linguistic conception of thought. On the other side, it is perfectly possible to construct a non-linguistic conception of thought that shall have the earmarks of behaviorism. Let us suppose, what to my mind is probable, that the simplest and earliest thinking is not linguistic in the least, but consists in adjustments of the individual to situations of novelty and difficulty—practical adjustments, not linguistic nor symbolic in any way. On the motor side, they should be looked for, not in the larynx, but in the hands and other organs of practical response. As thinking becomes more abstract and less closely bound to an actually present situation, these practical motor adjustments persist in some degree, and constitute the framework of all thought responses. With the development of speech in the individual, numerous linkages are formed between the practical and the linguistic responses, yet this does not mean an identity of the thought adjustments proper with the linguistic responses, since often an individual handles a situation intelligently without being able to tell in words what he has done or what he plans to do.

Such a conception, whether more true or not than the linguistic conception of thought, would square equally well with the behaviorist's general predilections. It would cert-

[1] PSYCHOL. REV., 1922, 29, 110.

ainly be a mistake to use the linguistic conception of thought as a touchstone in deciding whether or not to call any one a behaviorist. Behaviorism does not stand or fall with any special hypothesis which behaviorists happen to like.

Behaviorism, as an 'ism,' appears to be a sort of rationalization of certain research projects that lie (or lay) outside of the conventional field of psychological work. It appears as a theoretical justification for certain lines of work which were really entered upon because, taken for themselves, and quite apart from any philosophical justification, they promised to bear good scientific fruit. In reality, the behaviorists, as individual investigators, require no such theoretical justification. They are amply justified by their works. Animal psychology is justified apart from behaviorism, physiological psychology is justified apart from behaviorism, the behavior study of infants is justified, by its works, quite apart from any behavioristic theory. One would be proud to be the author of any one of numerous investigations made by behaviorists. One need not be a behaviorist to admire them. One need not have been a behaviorist to have made them.

In short, if I am asked whether I am a behaviorist, I have to reply that I do not know, and do not much care. If I am, it is because I believe in the several projects put forward by behaviorists. If I am not, it is partly because I also believe in other projects which behaviorists seem to avoid, and partly because I cannot see any one big thing, to be called 'behaviorism'—any one great inclusive enterprise binding together the various projects of the behaviorist into any more intimate union than they enjoy from being, each and severally, promising lines of work in psychology.

A BEHAVIORISTIC ACCOUNT OF INTELLECT AND EMOTIONS

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Behaviorism is put to the necessity of describing the traditional concepts of 'the intellect' and 'the emotions' in terms other than those of faculties or of structures. Following primary behavioristic assumptions certain requirements of description become apparent. Two of these assumptions are especially pertinent: (1) that if anything mental is going on in the organism, there is a three-fold process of sensitivity, conductivity, and contractility; and (2) that the organism tends primarily and continually to work as a whole, that its commonest impulse is to act 'all in one piece.'

The object of this paper is to suggest that the terms 'the intellect' and 'the emotions' are inadequate for psychological description, being encrusted with meanings, drawn from literature and common speech, which point to faculties rather than to actions or states; that these concepts used as substantives are ambiguous and inaccurate; and that references to the phenomena that travel under these terms would much better be made in adjectival form; that these concepts are not properly nominative, but primarily adjectival; that 'intellectual' and 'emotional' has a better denotation than 'the intellect' or 'the emotions.'

Two attributes are especially significant in considering intellectual and emotional behavior: *extensity* and *intensity*. Beginning, then, with the assumption that mental processes are described in terms of what the organism is doing and are described best in terms of the behavior of organism as a whole, we find that typical mental states are extensive in spread over the organism involving tensions and tonicity over a wide-spread area, while at the same time they tend to centralize intensity in one part of the body, as in the head or

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hands or legs or thorax. Then, too, another typical condition is that a high degree of intensity in any one part of the organism tends to increase the tonicity and tension of muscle systems throughout the other parts. Thus a very common type of response is that in which some field of the anatomy is tensed highly and the rest of the organism excited in varying degrees according to distance from the seat of the highest tension.

Holding in mind, then, the tendency toward totality in human activity and the necessity always for tension and tonicity in everything mental, as well as the necessary interrelation between extensity and intensity of muscular processes, we can suggest an understandable relation between 'the intellectual' and 'the emotional' by saying that what we call intellectual processes are those which tend to intensify tonicity and movement in a narrow and restricted field, while what we call emotional processes tend to raise or lower tonicity over an area approximating totality. Intellectual behavior reveals eyes, speech mechanism, hands, or some other selective part working at a high tonus, with other parts tapering down to lower tensions. Emotional behavior, on the other hand, reveals all these parts approximating equality of intensity.

Threading a needle is a simple example of intellectual process; it requires close attention, concentration, thought, deliberation—the commonly accepted marks of intellectuality. It demands high tension in eye muscles, neck, arms, and hands. When done skillfully and with precision it conforms to the common tests of an intellectual act. But during the time it is being *learned*, the process can be highly emotional, causing deep concern and excessive agitation. A boy or girl while learning to thread a needle is likely to reveal activity of a high intensity all over—the indication of the emotional nature of the behavior. Behaviorism submits that the totality of action likely to be involved, evidenced in the twitching of the face, setting of the jaw, humping of the shoulders, arching of the back, and even the writhing of legs and the shuffling of feet, is emotional, and that the actions themselves are the emotion. The same picture can be had

from a small boy learning to write. Writing is ordinarily considered an intellectual process. Yet for a child trying to write it is an intensely emotional experience. In order to master the special writing mechanisms, the child begins by trying to write with his whole body, crowding his head down into his neck, arching his back, twisting and writhing with his legs and feet. In so far, then, as either threading a needle or writing, or any other so-called intellectual act, tends to increase the tonus of muscles distant from the necessary mechanisms to those not so necessary, the act is in part emotional; not only to the perceptions of an observer but also to the subjective experience of the performer.

Reverse the picture as an aid to studying the situation that is clearly emotional. Assume a man in a forest frightened almost out of his wits (observe the significance of the language of every day as used here, 'frightened out of his wits'). He is agitated all over; his head shakes, or nods, or turns; his face reveals slack tonicity or else tonicity in excess; his breathing is very intense; his abdominal muscles are very tight or very loose; his knees shake; and his feet pull in two directions at once. Yet we are assuming that he has not quite lost all his wits; for if he has lost them utterly, it will not be through his intellectual acts that he escapes the danger that confronts him. Assume, then, that he has wits enough left to extricate himself. He perceives visual objects clearly enough so that the stump is not taken for a bear; he avoids stepping into the bog; he perceives an open space in the woods; he does not run into trees; and his hearing tells him that he is not actually being overtaken. So long as he can make such perceptions and discriminations he is not totally emotional; his actions are partly intellectual. While he may be tense or slack all over he still retains a concentration of higher intensity in the mechanisms that make possible these perceptions.

Herein then is offered a hint of the essential difference between intellectual and emotional states; the intellectual state is registered in the organism by a high degree of intensity locally; the emotional state is registered by a high or low

degree of intensity all over the body. The teleology of these two states is found in biological survival and social adaptation. The intellectual type of tonicity serves its purpose biologically in enabling man to make discriminations necessary to sustain life in complex situations when the ability to differentiate is vital to life. The emotional state determines wherein he is fit to master unusual or unforeseen situations, or whether he must succumb to the universe about him by failure to reach out with necessary vigor or withdraw with necessary reserve. Without states of widespread tension or of slackness, man would lack energy when energy and aggressiveness has survival value and would lack caution when survival value demands withdrawal and restraint. After a man has so fitted himself for his environment that he can secure tonicity in the right part of his body and of a serviceable degree, he is able to survive. If, however, he indulges in excess of widespread explosions at a high intensity or in extreme or continued deflations at a low tonicity, without compensating concentrations of intensity, he will not successfully adapt himself to his universe.

This is illustrated in such emotional states as we know by the common names of *fear* and *hate*. A state of fear strictly emotional would be a state of panic, and thus biologically dangerous. Wherein such a state is less than panic, it takes on intellectual aspects by way of tensions that make for clearer vision, more acute hearing, readiness to spring out of danger. When the clearness and the acuity and the readiness predominate over the panic, then the state approximates the intellectual. So with hate. If it is total and complete, it is destructive even to the point of jeopardizing survival. Wherein it is cautious, considerate of consequences, discriminative between valuable acts and acts of no value, it is intellectual. If the whole organism is compounded of tensions making for hate, it is in danger; if it is in part guided by tensions making for perception, for making concepts or recepts, or for the ability to differentiate objects, it has a chance to survive. The intellectual enters to offset the emotional. So with other common emotional titles, as jealousy, envy, love, sorrow.

The development of this joint sovereignty is illustrated in the learning process of babies. A baby can be said to be at first almost wholly an emotional being, because whatever it does it does all over its body. It begins with almost complete randomness of movement of equal intensity in all parts. For the baby the process of learning—the process of growing in intellectuality—is a process of overcoming complete randomness of movement, of shifting the center of intensity from total spread to intensity only in parts. An untaught baby trying to get hold of a bottle works just as hard with his feet, his abdomen, and his scalp as he does with his hands and fingers. When he has learned to pick up the bottle, he has learned first to apply greater intensity to his arms and fingers than to other parts, and then later to reduce the random movement of his extremities to the point where the tension is at least not visible. Yet it is doubtful whether the baby ever, in a matter of such biological significance as getting food, gets to the point of picking up its bottle and drinking without also tightening the muscles of his toes and grimacing and squirming in his abdomen.

This suggests a general rule, then, for predicating the relation between intellectual and emotional processes: the more a given activity makes for biological survival, or has to meet survival conditions, the more likely is the action to reveal clearly both intellectual and emotional aspects. Thus always in the securing of food, especially in the satisfying of hunger, in matters of self-protection, in sex relations, and in the freedom of the body to move about and to grow successfully, there will be at once this combination of selective and discriminative tensions constituting intellectuality and also the wide-spread tension that constitutes emotionality.

So it comes about that the most serviceable states and activities of life are never simply intellectual or simply emotional, because the body is built to make total localizing difficult and total extensity dangerous. In affairs that matter biologically, where the issues of life are at stake, wherein man is genuinely struggling to adapt himself to his universe, there is always necessity for both specialized reaction and for

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response approximating totality. The man who is excessively emotional is neither biologically safe nor adapted socially; while the man who is excessively intellectual frequently finds himself a social hermit and is all too often a poor specimen for survival. The type of man most commonly approved by society, however, is the man of great energy and action with clear-cut powers of observation, discrimination, and deliberation; or reversely, the man of clear powers of perception who yet can 'throw his whole being' into whatever he is doing.

Another picture of the same situation we have in what we call the alert body. The alert body is one in which the activity is intense enough and wide-spread enough so that postures and movements even reveal to an observer the widely spread tension. This state is highly serviceable biologically; it gives the organism the capacity for perceiving many objects at once. What we call presence of mind is the ability to meet a situation which stirs wide-spread activity thus tending toward emotionality, but at the same time includes the ability to observe accurately several objects at the same time; or, better, to observe clearly an object of unusually intricate and diverse pattern. This state is highly intellectual, but also highly emotional. It is the organism working at its most efficient level.

One type of man commonly conceded to be of the highest capacity is the orator. To succeed he must know his subject clearly, must be able to select words with precision and delicacy, must know what his voice can and will do, and must have command of his arms and head and general bodily activities. Whatever else he is, he is an organism exhibiting unusual control. But at the same time if he really is an orator and is competent to command the 'ear of listening senates' or to control the general crowd, he is intensely animated all over his body. The tonicity of his muscles from toe to crown is as great as if he were performing strenous labor; his facial muscles are in full play; his jaw opens wide or sets hard; his head moves with vigor and decisiveness; his shoulders sit erect, his chest heaves, his abdomen is at

work in every muscle; his arms and legs and feet are eloquent visibly. In other words, if he is a good public speaker, he reveals an approximately even balance between the power of careful discrimination and the power that comes from total activity. Whatever may be the degree of activity all over his body, still there is a specialized activity of the muscle systems which enables him to apprehend concepts, to put them into words, and to make his language rightly meaningful to the people before him. He is thus both intellectual and emotional.

In general the ideal type of man, the man biologically and socially secure, is the man who has mechanisms in all his body that are capable of both high and low tonicity, but who is schooled to regulate the tonicity locally to serve the social or biological need at hand. The best of executives, manifestly intellectual men, reveal also great vigor of bodily activity, sometimes explosively applied—they have strong and definite emotions. The great conquerors in history, from all we can perceive of them, seem to have been men with the ability to react as discriminating mechanisms at the right time and in the right degree, while at the same time they were capable of reacting with extreme vigor or reserve of total activity.

In sum, then, we may state this difference not in nominatives, 'the intellect' and 'the emotions,' but in adjectives, *intellectual* and *emotional*. Intellectual states are those making for selection, appreciation of likeness and difference, discrimination, by localized and therefore controlled intensity. Emotional states are those making for unselective and undiscriminative responses, therefore uncontrolled, by means of high or low intensity of wide extensity. Thus it is meaningful to say, what the language of every day speech also says, that intellectual activities are practically synonymous with perception—knowledge, apprehension, the appreciation of likenesses and differences; while emotional states are the opposite—the disposition to react with these processes left out. Inasmuch as all of us start life totally emotional, with almost no powers of discrimination, our education, as is commonly agreed, is a process of gaining powers of discrimination and selection.

Behavioristically, this is a process of reducing the spread of local tensions; though practically all local intensities extend, at lower degrees of intensity, to the whole organism. The typical human organism may at any given moment be predominantly intellectual or predominantly emotional, predominantly selective or predominantly unselective, predominantly tensed locally or all over; or it may secure a balance between the two. Farther than this it is difficult to see how these concepts, useful as they are in every day speech, are serviceable to the student of mental processes and human behavior.

THE UNVERBALIZED IN HUMAN BEHAVIOR

BY JOHN B. WATSON
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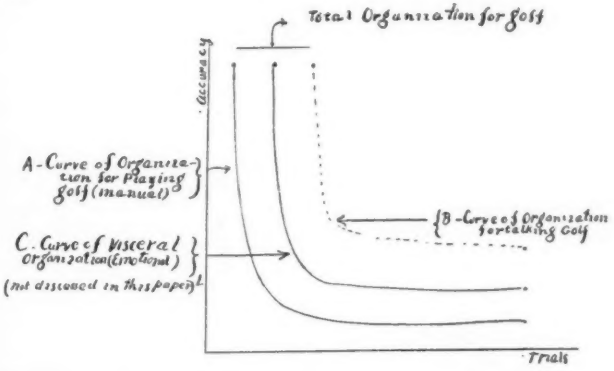
It requires possibly no argument to convince the genetic psychologist that under the influence of social demands the developing human organism does verbalize each and every one of his manual habits—including under 'manual' all striped muscle organization except the laryngeal.[1] Possibly it is more nearly accurate to say that he puts on simultaneously a parallel verbal habit each time he forms a general bodily habit. A man learning to play golf learns (usually) simultaneously to talk golf. We may illustrate this by a simple sketch:


Total Organization for Golf
Accuracy
A-Curve of Organization for Playing Golf (manual)
B-Curve of Organization for talking Golf
C. Curve of Visceral Organization (Emotional)
(not discussed in this paper)[1]
Trials

Oftentimes it is more important for the business man to talk golf, hunting, fishing and the like than to be able to exhibit proficiency in them. He can always refuse to go golfing, hunting or fishing when his lack of manual skill is not equal to his verbal performance, but he cannot refuse to talk about the technical points of these avocations and stay in the athletic group.

[1] The discussion of visceral organization will appear in a forthcoming number of the PSYCHOL. REV.

Furthermore, since so many millions of man's adjustments are verbal, the verbal organization soon comes to be dominant.[1] The verbal process soon comes to initiate and control all of the arm, leg and trunk organization.[2] Watch the golfer as he makes a bad shot; ask him what he did wrong. If you are a lip reader you can in many cases read his words without asking him any questions. "I stood too close to the ball. I've got to learn to stand back. I bent my legs; I didn't follow through." Watch him address the ball for his next shot. He says to himself, "Stand a little back," and he steps back, etc. The verbal organization, in addition to its usefulness in getting notice in the club house, is on the field an intimate part of the total organization called into action when playing golf.

The behaviorist believes that the verbal process whenever it is present is always an actual functioning part of every act of skill.

'Memory' is really the functioning of the verbal part of a total integration. It should be granted the behaviorist, then, that the verbal organization paralleling the overt muscular organization is there to play a principal part in the total reaction ('control,' initiation, correction, modification, etc.). It plays another rôle, a social one—the rôle of the introspectionist's 'memory.' Once we have verbalized a bodily habit we can always talk about it. If you couldn't talk about golf, the only way you could prove or exhibit your organization in it (your 'memory' of it) would be to go to the golf field and play it out hole by hole. But the situation for touching off your verbal organization on golf occurs a thousand times more frequently than does the actual situation touching off your organization for playing golf (simultaneous presence of links, leisure, clubs, golf balls, companions, clothes, plus bodily and verbal set—'I am going to play golf now'). This

[1] See Lashley's article, PSYCHOL. REV., 1923.

[2] A fact which, if it had been grasped by the introspectionists, would have saved them from considerable perturbation. For example, when they call themselves parallelists on the first page and use interaction all through the rest of their text; when they try to make 'consciousness' do something—correct an error in a habit—or fix the happy accidental successful movement when a new habit is being formed by the trial and error process.

talking about our manual organization is what is popularly meant by 'memory'—*that is, the running through or exhibition of the verbal part of a total bodily organization.* It should be pointed out that if one other part of the total organization ('A' in our diagram) should become operative under appropriate stimulation (of the golf field), the individual's organized response with the club is just as good a demonstration of 'memory' as the verbal talk about golf.

Organization without Verbal Parallels

It is obvious that a great deal of organization goes on without verbal parallels, viz., (1) all organization put on in infancy; and (2) all organization put on throughout life in the visceral or emotional field (unstriped muscular and glandular organization).

Organization in Infancy

The recent work on infants (the author's own and the work in progress under the auspices of Teachers College under the joint direction of Mrs. Mary Cover Jones and the author) seems to show that an almost unbelievable amount of organization goes on in infants too young to talk. This shows itself not only in the overt organization of arms and legs and trunk, bodily habits, but equally well in the visceral field, conditioned fears, rages, loves, taking the form of strong attachments to mother or nurses, tantrums, negative reaction to people, and the like.

Our observation shows that the infant cannot before thirty months of age (and how much later we cannot say—possibly very much later) parallel each unit manual habit with a corresponding word habit. Today in front of me is a child two and one-fourth years of age. He can speak, under appropriate stimulation of object or situation, possibly five hundred words, but sentence formation is of the level of 'Rose take Billy bye-bye,' 'Put Billy's coat on,' etc. He is still at the age of incessant repetition of words and sentences. When the nurse brings him in, the father says: 'What did you see, Billy?' and he says: 'What did you see,' etc. In contrast, this same infant learned to manipulate at two years of age a

rather large, pedalled 'kiddy car,' to propel it, guide it, mount it, coast down hill, drag it up inclines and along the sidewalk, and to fly down. He reacted against help, would fall off without crying, mount and start over again. The *only verbal* parallel is, 'Billy ride kiddy car.' There is no verbal organization which you can call out relating to turning the bar to the left or right when he turns to left or right; about pedalling being harder uphill than down; the greater the incline the more rapid his speed, and the like. Yet overt, non-verbal responses are perfect, even after weeks and months of no practice. This example from hundreds of others shows that the manual habits of three-year-and-under infants are unverbalized. The only way you can show 'memory' or organization in such cases is to put the child in the situation where he can exhibit that bodily organization. Contrast this with the three-and-a-half to four-year-old who goes walking, to a party or to a movie, or who takes a railway trip, and talks you blind, deaf and dumb about it.

The Weakness of the Freudian's Assumption of the Unconscious

The Freudians claim of course that the childhood memories are lost because childhood is an age where free, spontaneous actions bringing 'pleasure' come under the ban of the social; society punishes and a painful repression into the 'unconcious' takes place; that these childhood memories are lost until the analyst gives the mystical phrase which opens the cave where the memories are stored. The unsatisfactory ground for this assumption is now apparent. *The child had never verbalized these acts.*

I have become totally skeptical of any so-called adult 'memory' antedating certainly the three-year childhood period. My skepticism comes too from observation of children and not through any presuppositions. Today I tested out a hungry two and one-fourth years old baby with a nursing bottle filled with milk. The test in detail follows:

Test on Memory for Bottle

Baby B, 2 years 3 months of age. February 17, 1924.

At 12:30 noon, the baby's meal time, his regular nurse picked

him up and said: 'Dinner, Billy,' laid him flat on his back in the crib as was her usual custom when he formerly was fed from the bottle. She handed him the warmed bottle just as she had one and one-fourth years before.

The baby took the bottle in both hands, then began to manipulate the nipple with his finger, then began to cry because 'dinner' with him at noon consisted of meat and vegetables. When told to 'Take his milk,' he put nipple to mouth and got taste of drop of milk and began to *chew the nipple*. Nursing could not be called out. He called to his mother and cried and handed her the bottle and raised himself to a sitting posture. Pushed the bottle toward mother, then toward father, with both hands. He was then let down to floor and good humor was restored.

He was told 'Jimmie drinks from bottle' (his infant brother). Then he took the bottle, stuck into his mouth and walked off *chewing the nipple* as he went. Nursing was completely gone.

Billy was nursed at the mother's breast for the first month and was then put wholly on the bottle.[1] At the end of nine months he was weaned from the bottle and made to drink from a silver mug. Until he was one year of age he drank his morning orange juice from a nursing bottle. He never saw a nursing bottle from that day until the test on February 17th.

Before the test took place every effort was made to stir up a verbal memory of some kind, but unavailingly. He was asked, 'Didn't you used to drink from a bottle when you were little?' Then he was told that he used to drink from a bottle. Then he was asked, 'Can't Billy drink from bottle?' etc. His behavior throughout was exactly that of reacting to a strange new object, forced to react to it when his whole body was ready to react to his regular food.

The test shows that not only was there no verbal organization around this once all important infant act that could be tapped but that even the manual organization (including of course sucking, etc.) was gone.

Thus infancy, where the process of 'repression' is supposed to bury so many unconscious treasures which come to light under the prestidigitation of the analyst, turns out to be a wholly natural kind of state. Bodily habits form normally,

[1] On the above date he was similarly given an opportunity to nurse from the breast. He could not be made to take the nipple into his mouth and soon began to struggle at being held in the lap in the nursing position.

both habits of avoidance and approach, and habits of manipulation; but the bodily habits are *lacking verbal correlates because the infant puts them on at a later age.*

It is believed that this aspect of Freud's unconscious can be adequately cared for along the lines indicated. The Freudians have no positive evidence to offer in controversion, at least they have offered none. I find no actual observation in their literature of the day-by-day life of the infant. Hug-Hellmuth's volume might just as well have been written without any infant being present, so inaccurate and *non*-scientific are its observations and assumptions.

Other Unverbalized Organization

Now that it has been demonstrated that conditioned visceral or emotional, responses are constantly forming from infancy on; that these conditioned responses are transferred to a variety of situations; and that they persist for long periods of time, possibly throughout life,[1] it behooves us to begin to think of man's visceral system as being subject to all the laws of habit formation.[2] We talk of kinæsthetic organization, of verbal organization, but rarely of visceral organization. One reason for this is of course social. Society makes no demands upon us, or few at any rate, to talk about unstriped muscle and glandular habits. When conditioned salivary reflexes are established in childhood, the child is never told about them; no demand is made upon man to verbalize elimination habits, habits connected with the slowing or speeding of the sexual organism. Few men and fewer women have paralleled their sex organization with words.

Again, what child has ever verbally organized its incestuous attachments? Not one. Nor has there been any 'repression' because society was not and is not organized to place youthful

[1] I have under observation a case of a youth of twenty-five who was bitten by a horse at two years of age. To this day he will not go near a horse, walking across the street to avoid being near one. His whole organized life is disturbed when in the presence of horses.

[2] In the succeeding article already referred to I shall take the position that the terms visceral behavior and visceral organization should be used in place of 'emotion' and 'emotional organization.' The recent flood of arm chair discussions on 'instincts' and 'emotions' have robbed these terms of their usefulness.

incestuous attachments under the ban (quite the contrary). Only a few days ago one of our most distinguished pediatricians, in condemning the idea of an experimental nursery, said, 'Infants need a mother's love, they should be danced at her knee, petted and made much of.' To tell a mother who is breeding habits of dependence in her child by letting it always play under her eye, who always feeds it herself (creating thereby a situation where a violent tantrum will occur if any one else ever has to feed it), that she is laying up trouble for the child when it has to break its nest habit, is to arouse a violent storm of protest.

Only a slight study of this field convinces the geneticist that a large part of our visceral organization goes on from infancy to old age without corresponding verbalization. Even an adequate list of words is absent and there is no social mechanism for the word conditioning of the developing subject. A little of it is verbalized. This comes about when the acts of belching, elimination, releasing of gas, masturbation, sex intercourse and the like are exhibited in the presence of elders. The psychological process of verbal conditioning takes the form, "You must not let your *stomach growl* in in company." "Run outside or cough to cover it up." "Say 'excuse me' when you do that in company." While many similar examples of verbalization in the visceral realm occur, verbalization is the exception, not the rule.

Such observations and reflections as the above lead the behaviorist to make the following general assumptions (with apologies for lack of better observational backing!):

1. That an enormous number of manual habits are formed, especially during infancy, without corresponding verbal habits.
2. That a still larger amount of visceral organization (organization in unstriped muscles and glandular components) is constantly forming without verbal organization, not only during infancy but also throughout life.
3. The assumption seems to be reasonably grounded that this unverbalized organization makes up the Freudian's 'unconscious.' (Another possible source of the so-

called 'unconscious' in line with natural science might be found in cases where for one reason or another the verbal organization is blocked, *e.g.* where there is simultaneously present a stimulus to say the girl's name in a love affair and one to remain silent. In such cases only the visceral organization appears, such as incoherent sounds, blushing and the like.) They likewise make up possibly the introspectionist's 'affective processes.' [1]

4. That the genetic rule, when the proper age is reached, is to put on simultaneously word and manual organization.
5. That once the verbalization of the manual begins, word organization soon becomes dominant because man has to solve his problems verbally. The word stimulus can thereafter call out any organized response in the organism or modify any activity already going on. For example, 'I must start to build that bookcase now,' or 'I am shooting too high; I must aim lower.'
6. That, that aspect of 'memory,' which is supposed by the introspectionist to be difficult for the behaviorist to cope with, is merely the calling out of the verbal parallel of manual habits earlier put on. Memory in the behaviorist's sense is any exhibition of manual, verbal or visceral organization put on prior to the time of the test.

It is believed that when subjective psychologists have given verbalization its due place in the whole process of bodily organization they will be ready to admit that being 'conscious' is merely a popular or literary phrase descriptive of the act of *naming our universe of objects both inside and outside*, and that 'introspecting' is a much narrower popular phrase descriptive of the more awkward act of *naming tissue changes that are taking place, i.e.* movements of muscles, tendons, glandular secretions, respiration, circulation and the like. In other words, neither of them has any existence as a psychological process. They must be looked upon solely as literary forms of expression.

[1] I am still inclined to maintain my earlier position, that tumescent processes (emotional) have been named '*pleasant*' and detumescent processes '*unpleasant.*'

INTELLIGENCE CONCEIVED AS A MECHANISM[1]

BY JOSEPH PETERSON
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The doctrine of associationism has furnished the main basis for mechanistic views of behavior. It originated as an explanatory theory from early physical conceptions of molecular vibration, which were carried over, especially by Hartley, in a rather wholesale fashion to the functioning of nerve elements. This doctrine has been very fruitful in psychology, and has done good service toward displacing animistic conceptions of spontaneous forces of various kinds and toward the establishment of scientific views of behavior. Many of its contributions are undoubtedly permanent.

Associationism has, however, serious weaknesses, some of which may be mentioned only. First, it makes the individual passive and largely subject to the mere order and intensity of external stimuli, whereas we feel ourselves to be active reconstructing agents. In the second place, emphasizing the piecemeal functioning of discrete units or nerve elements, it has not satisfactorily squared with such phenomena in experimental physiology as inhibitions and facilitations among different organic processes nor with the similar action of higher nerve centers on lower ones. Finally, it has failed rather significantly to account for the selectiveness in phenomena of attention, reasoning, and learning. This failure has been so obvious that high authorities in psychology, even though themselves committed to the general principles of association, have inwardly rebelled at the picture that it gives, of these selective processes and of volition, and have invoked 'spiritual forces' or various faculties as supplementary. Recently a recognized authority [2] has vigorously and openly condemned mechanistic conceptions in psychology, and he

[1] Read before the American Psychological Association in Madison, December 27, 1923.

[2] McDougall, Wm., 'Outline of Psychology,' 1923.

has definitely departed from such conceptions in a book that is bound to have much influence toward the perpetuation of mysticism and the support of a popular conception which is strong enough to have come to forceful expression [1] in one of our leading educators. There is no question that this view can find much support in many of our texts where faculties and mystic forces are frequently assumed to supplement the neurological conceptions built up in earlier chapters. Mechanistic conceptions are, therefore, by no means universal among psychologists, nor are they consistently held by those who do accept them in principle. This condition is doubtless due in large part to the fact that present mechanistic conceptions have been taken over too directly from mere inanimate processes.

The all-sufficiency of association as an explanation of behavior has long since been questioned. Even though the explanation of behavior on the basis of habit was one of its strong points, it was precisely in learning phenomena that its weakness was early evident, and as a consequence pleasure and pain—either as mental forces or as physiological conditions—were invoked as supplementary factors.[2] More recent experimental tests of the efficiency of association factors in learning have gone against the doctrine.[3] Every attempt that has been made to check up quantitatively on frequency and recency influences in learning has shown that learning does not follow expectations based on these factors. Attempts to keep constant the additional factor of vividness or intensity—whether intensity of stimulation or of response—have brought no different results. Associationism as conceived at present simply does not explain constructive learning in which the elimination of so-called errors is involved.

[1] See Bagley, W. C., 'Educational Determinism; or Democracy and the I. Q.,' *School and Soc.*, 1922, 15, 373–384.

[2] Morgan, C. L., 'Introduction to Comparative Psychology,' 1894, Ch. 12, and Thorndike, E. L., 'Animal Intelligence,' Psychol. Monog., 1898, 2. No. 4, 103 *ff.*

[3] Peterson, J., 'Frequency and Recency Factors in Maze Learning by White Rats,' *J. Animal Behav.*, 1917, 7, 338–364; 'Learning When Frequency and Recency Factors Are Negative,' J. Exper. Psychol., 1922, 5, 270–300. Kuo, Z. Y., 'The Nature of Unsuccessful Acts and Their Order of Elimination in Animal Learning,' *J. Comp. Psychol.*, 1922, 2, 1–27.

Even the rat in the maze gives serious trouble to the associationist, according to whose doctrine the animal could do nothing but repeat its errors. It could never eliminate them. Quantitative tests have likewise brought negative results for association in the rational learning of young human subjects.[1] It is indeed remarkable how completely and regularly our long cherished doctrines of the influence of frequency and recency factors in the modification of experience fail in these learning processes, and how indefinite and vague has been the conception of intensity as a factor in association.

What means have we, then, of accounting in scientific terms for the modification of behavior in learning involving the dropping off or elimination of errors, and for the selectiveness seen in all higher forms of behavior even more strikingly than in learning? To say, as some of our texts do, that a conscious intelligence comes in and effects the adjustment when habits and instincts fail, is to introduce a *hocus pocus* factor comparable to the animism of the ancients; and the assumption of faculties takes us no further. Note that our Freudian friends and popular writers in general have even assumed that where a conscious intelligence does not enter to effect these controls, an unconscious intelligence—*i.e.* unconscious to us—must do it. They have taken up the fallacious view, so frequently found in our texts, of ascribing some arbitrary force to intelligence or to consciousness.

Now intelligence, in the view here taken, is not a force at all; it is not a power that effects adjustment, but is only a mechanism through which or by means of which the adjustment is brought about. For our purpose we shall define intelligence as a biological mechanism by which diverse impulses are brought together and given a somewhat unified and consistent effect in behavior. These impulses are, of course, the results of stimuli [2] of various kinds—interoceptive,

[1] An unpublished study of the responses of several hundred white and negro children, by Miss Pantha V. Harrelson.

[2] The term 'stimulus' here is used in a broad sense to include any exciting factor either external to or within the organism. The point of view of this paper is similar to that expressed by Dr. Thurstone in his article, 'The Stimulus-Response Fallacy in Psychology,' PSYCHOL. REV., 1923, 30, 354–369, except that his use of the term stimulus is very much restricted. He may not, however, be willing to subscribe to the

proprioceptive, and exteroceptive—both simultaneous and successive, direct and indirect. It is then the *stimulating situation*, external objects and interorganic changes resulting partly from general metabolism, that forces the adjustment in intelligent as well as in reflex and automatic acts. Consciousness is but an aspect, peculiarly intimate to the individual himself, of the adjustment; it is a peculiarly direct aspect of the relation between the individual and the situation to which he is responding, not something apart from these things that regulates behavior.

Taking this position we rid ourselves of certain difficulties, especially of those relating to dualism and mysticism, and we are forced to conceive of some sort of mechanism that will be free from the limitations of associationism already indicated, one that will account for our feelings of activity and for the organizing and reconstructive functions usually attributed to intelligence, as well as for the more simple calling up of one experience or habit by the stimulus that has been associated with it. Since a mechanistic conception need not rest upon any one simple principle, there is no reason why we should give up the conception of association in so far as it is useful and is supported by empirical tests. Mechanism as here used means simply an arrangement of the different parts of the organism, both inter- and intracellular, whereby all the life processes may by interstimulation and by arousal from external objects be kept in properly balanced operation and adjustment to environment. Interstimulation among the different part-processes of the organism may be either facilitating or inhibiting, in all conceivable degrees, so that responses to external situations may be properly coördinated.

The position here taken definitely assumes that behavior, defined broadly enough to comprehend all our conscious experiences as well as unconscious acts, is completely determined by organic structure as affected by internal and external stimulation. It is only an abstract, absolute determinism,

idea that intelligence is but a mechanism. There is no question that we should put more emphasis than has been put on the self-activity of the organism, but this activity is doubtless due to inner stimulation from metabolic changes as mentioned briefly above.

an artifact of the philosophers, that people shudder at. Every sane person accepts in his dealings with associates the kind of practical determinism we are assuming. The question, often asked, whether a complete knowledge could predict every event in the universe a thousand years in advance of its happening, is misleading and useless and hides within itself a dualism that we are trying to avoid; such knowledge is obviously impossible, and the prediction itself would have to be part of the events. Knowledge is better regarded as an acquired readiness for responses of certain kinds to given conditions, than as an apprehending of things in the universe by an outside agent; it is not something added to the universe, but rather a modification of an organized part of the universe by other parts. Even though the aim of psychology is to understand and control behavior, in a world so inconceivably complex as is ours, all that can ever be accomplished is control within certain narrow fields and for short periods of time. Nor need we fear that the constantly growing hope of science for greater control of events by ourselves is going to curtail our own freedom and discourage our ambitions. This hope has been strengthened in late years by discoveries and achievements that no one could have predicted a century ago.

We cannot elaborate here on the detailed workings of our intelligence mechanism, but can only suggest very briefly the underlying principles. It is unquestionably wrong to conclude from the rate of the nerve impulse and the shortness of reaction time as measured in our laboratory experiments, that the effect of a stimulus is but momentary. On the contrary there is ample evidence, both subjective and objective, to suggest that the effects of the several stimuli are held over in the organism in some way so as to join with one another and operate collectively. By this means the responses become functions of certain more or less complex situations and probably never of any one single stimulus. Sensory impulses do not simply go out through higher centers to muscles and glands, bring about responses, and thus end their effects. The responses which they arouse set up further

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sensory or proprioceptive impulses, which in turn pass through higher coördination centers and thence to effectors, bringing about other responses and adjustment tensions; and so on. In this manner, and probably also by more direct inter-stimulation of autonomic processes, the effects of stimuli persist for a time—often for long periods—and enable the organism to suspend action and respond with more organic completeness to the general consistency of the situation, both inner and outer. Behavior, moreover, instead of being a passive receptivity to stimuli, is motivated by the urge of inner organic processes and general metabolism, and external conditions are but contributory stimulative and directive agencies.

The individual is therefore intelligent in proportion to the extent of his ability to take in the effects of the related factors in relatively large practical situations. But different individuals are doubtless disposed to respond differently by innate constitution to the various aspects of these situations—the social, the immediately objective, the symbolical and remotely objective, the unitary, the abstract and analytical, etc.—and by different degrees of directness, and so we obtain a corresponding variety of intelligences. The freedom of any individual is measured by his relative independence of the order of stimuli and of the conditions immediately about him, and is evidently dependent on his heredity and past training. Such freedom is not incompatible with predictability nor contradictory to the laws of science.

The recent work of cytologists and experimental geneticists is certainly such as to strengthen our faith in mechanism, and at the same time it reveals a delicacy of organization that raises in our minds a caution against a too ready acceptance of a grossly materialistic mechanism. The laws of life must be found in life, not in mechanics.

William James, in a letter [1] to Henry James in 1910, in which he is trying to make clear how freedom is possible, abandons his usual appeal to a non-predictable spiritual force and argues in terms of a mechanism. "Physically

[1] James, Henry, 'Letters of William James,' 1920, Vol. 2, 345.

a dinosaur's brain may show as much intensity of energy exchange as a man's," he says, "but it can do infinitely fewer things, because as a force of detent it can only unlock the dinosaur's muscles, while the man's brain, by unlocking far feebler muscles, indirectly can by their means issue proclamations, write books, describe Chartres Cathedral, etc., and guide the energies of the shrinking sun into channels which never would have been entered otherwise—in short, *make* history."

THE PHENOMENOLOGICAL POINT OF VIEW[1]

BY PAUL THOMAS YOUNG
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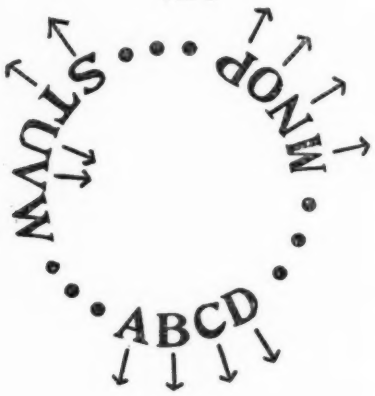
The present paper is an attempt to develop a phenomenological point of view towards the subject-matter of psychology, and to show how experience may be regarded as a relation discoverable within phenomena. I shall begin by distinguishing several fundamental classes of phenomena.

FIG. 1



Let *A B C D* . . . (Fig. 1) represent the class of physical objects observed during waking life: stones, houses, stars, etc. Let *M N O P* . . . represent a special group of physical objects which, on the basis of certain characteristics, are usually called living organisms: trees, bacteria, men, etc. Strictly speaking *A B C D* . . . and *M N O P* . . . con-

[1] Read before the American Psychological Association at Madison, Wisconsin, December 27, 1923, under the title 'The Foundations of Psychology.'

The paper aims to state a point of view so fundamental that all schools of psychological thought can accept it—a point of view to which it is possible always to return from hair-splitting discussions. It is not the aim of the paper to outline a program for psychology.

stitute a single series of physical objects. Sciences such as astronomy, geology, chemistry, and physics study the phenomena represented by *A B C D. . . .* The biological sciences study *M N O P . . .* or else the interrelations of *M N O P . . .* with *A B C D. . . .* All the objects symbolized by the series *A B C D . . . M N O P . . .* appear to be *external* to the person who observes them and they seem to *exist in their own right* independently of any particular observer. This external character is represented by arrows pointing outward. The physical sciences study objects, events, relations, and processes *as if* there were a real order of existence independent of any particular observer and external to the given order of phenomena.

Let *S T U V W . . .* represent the class of phenomena commonly called 'mental': dreams, hallucinations, feelings, emotions, desires, purposes, beliefs, etc. Some of these phenomena—dreams and hallucinations, for example—bear the meaning of objective reality [1] and in this respect they are indistinguishable from *A B C D . . . M N O P . . . .* The outward-pointing arrows of *S* and *T* indicate this objective character. Other members of the class—feelings, emotions, desires, beliefs, intentions, etc.—sometimes have a personal reference. This reference to myself is indicated by arrows pointing inward at *U* and *V*. *W* represents a so-called 'mental process' directly observed by a trained psychologist without either the objective or the personal meanings. As an example of a *W* consider a tone. It is a bare, on-going experience; it is one of the givens; it is ultimate.[2] The tone

[1] Although three years have elapsed since the experience, I vividly recall seeing a white form enter my bed room and glide noiselessly to the clothes closet. The form was unmistakably that of a burglar intent on robbery. I suddenly awakened to find my heart thumping, my body covered with cold perspiration, and the full moon shining through the window directly upon the pillow. This particular dream was every bit as objectively real as any normal waking experience. The white form, the apprehension of its significance, the emotional tenseness, were given with the vivid objectivity of waking life. When I was thoroughly awake I realized that it was only a dream; *then* the utterly silent and swift movement seemed fantastic.

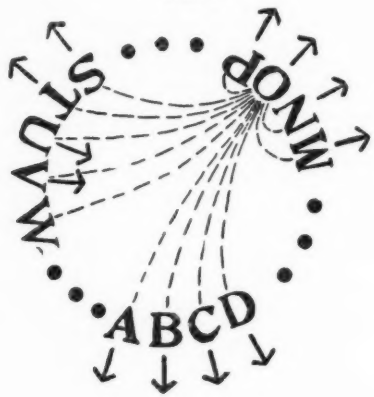
Since this dream occurred shortly after falling asleep, it was probably an interpretation built around an after-image conditioned upon previous stimulation by the moonlight.

[2] The question 'What is the tone?' seems to imply that it is a material thing,

may be observed, and it may be described by comparing it with other 'mental processes.'

The three groups of phenomena represented in Fig. 1 are not sharply marked off from each other. The distinctions are made for convenience only, and their value is pragmatic; they aid in handling a complex situation.

FIG. 2



Let *O* (Fig. 2) represent my living organism, my bodily self. *O* is a member of the biological series *M N O P*. . . . It is well known that the phenomena called 'mental,' represented by *S T U V W* . . . , are related to *O*. These phenomena are commonly called *experiences* of *O*, and they are explained by reference to *O*. Let the broken lines from *S T U V W* . . . to *O* represent the experience relation.

The term 'experience' is used here to include all phenomena *just as they are given* (*A B C D* . . . *M N O P* . . *S T U V W* . . .) whenever these phenomena are regarded

and there is good evidence that it is a process rather than a thing. The question 'Where is the tone?' may lead to localization in the head or out there or here where I see the tuning fork. The meaning of locality is added to the bare process.

Other questions lead more obviously to interpretation and to philosophical speculation. Granting that the tone is a process and not substance, the process *must be a process of or in something*. Hence the question 'In what substance does the process go on?' Various answers are forthcoming: the tone is vibratory motion of a tuning fork or of the air; the tone is a neural reaction; the tone is an experience of my soul. Every answer goes beyond the bare datum.

as existences depending upon *O*, or somehow related to *O*. To call a phenomenon an 'experience' need not change it in any way. The term 'experience' merely names a relationship. The relation in question may be expressed by the simple formula $\Phi = f(O)$ where Φ represents any particular phenomenon *just as it is given* and where *O* represents the psychophysical observer of the phenomenon.

The sum total of phenomena, represented by *A B C D . . . M N O P . . . S T U V W . . .*, depend for their existence upon *O* in such a manner that nervous disease, accident, anæsthetics and other conditions may bring about a complete lapse. All phenomena, taken just as they are given, with the meaning of objective reality, the personal meaning, and with many other characteristics, are related to my body and more particularly to my nervous system in such a manner that if my nervous system be destroyed, all phenomena (so far as I know) cease; if my nervous system be injured, phenomena may be disorganized; if my nervous system be subjected to drugs or to knife, phenomena may be modified or they may vanish.[1] These relationships of the whole to a part are represented in Fig. 2 by broken lines. *O*, my bodily self, is an object resembling the others; and it too disappears during dreamless sleep.

The organism, *O*, of Fig. 1, is a *physical* organism. It has mass; it moves when stimulated from without and when stimulated from within. Its behavior depends upon physical conditions. The organism, *O*, of Fig. 2, is a *psychophysical* organism. It is an organism which sleeps and wakes, which understands, believes, thinks, desires, expresses emotions, sees objects and events, hears sounds, etc. In a word, it *has* experiences and the having of experiences marks it off from the purely physical organism of Fig. 1. When I say that an organism *has* an experience or that an organism is in the conscious condition I mean merely that for it some phenomenon

[1] A corollary to this proposition is that for every discoverable characteristic of experience there is a corresponding characteristic of the body. Every observed difference of sensory content presupposes a difference of some sort within the body. Every emotion, every intention presupposes bodily conditions.

or other is given,[1] that some *A B C D . . . M N O P . . . S T U V W . . .* is present which in some sense or other may be observed.

The failure to distinguish sharply between the purely physical organism of Fig. 1 and the psychophysical organism of Fig. 2 is at the root of a good deal of careless thinking regarding the program of psychology. It is at the root of every behavioristic program which closes the eyes to facts of human experience. Let me dwell upon this point a moment.

The behaviorist of the severe type places his faith in the postulates of physical science, believing them to be fully adequate to account for all facts of behavior. He aims to give a complete account of animal movements in terms just as objective as those used by a mechanic in describing the behavior of a locomotive. This task is straightforward and important for psychology.

So long as the behaviorist holds consistently to the physical point of view no one can quarrel with him. When, however, he claims to account for the facts of experience by a system of postulates which at the start eliminates all reference to the experience relation the orthodox psychologist demurs. If the student of behavior held consistently to the physical point of view, questions regarding the existence of the 'supposedly unique facts of consciousness'[2] would not arise. An analogy will make the point clear.

Let us assume, for the sake of argument, that there are no cats and let us proceed to develop a science of the catless world. The assumption works beautifully; it describes 99 percent of the world. Surely the assumption is universally

[1] From the very nature of the case I cannot observe or in any way experience the unconscious condition; this condition is necessarily assumed. When any phenomenon is given, *i.e.* when some datum is there, I am conscious. The difference between the two conditions is typified by the contrast of dreamless sleep and waking life. For me the difference is an all or none affair.

For the physical scientist the conscious condition is necessarily inferred. It is never a datum. Hence if we are to raise the question of the nature of the conscious condition and its relation to the unconscious condition, we must at the start assume the validity of physical and psychological points of view.

[2] Lashley, K. S., 'The Behavioristic Interpretation of Consciousness,' PSYCHOL. REV., 1923, **30**, 237–273; 329–354. The quotation is from page 240.

true! There are no cats! But wait! Some fine day without any warning a curious animal crosses the path and our friends who are of a different faith call it a cat. This puts us in a rather embarrassing dilemma. We may maintain that the animal in question is not a cat and straightway give it another name; or we may admit that the animal is a cat and hurry to explain that we are not especially interested in cats and that we will study the world *as if* there were no cats. In the latter case we have recognized our postulate as a postulate.

The analogy, I think, is an exact one. To the psychologist the physical point of view is negative. It intentionally eliminates the observer and so far as possible disregards the experience relations represented in Fig. 2 and necessarily the group of phenomena represented by *S T U V W* . . . in Figs. 1 and 2. The physicist examines phenomena *as if* there were no beliefs, thoughts, emotions, intentions, dreams and other experiences. He describes phenomena *as if* objects and events existed in their own right external to any particular observer and independently of him. He methodically eliminates 'errors of observation' and the 'personal equation.' If, therefore, he wishes to inquire about the phenomena *S T U V W* . . . and more specifically about the experience relations represented by broken lines in Fig. 2, he must at the start admit the validity of the psychological point of view.

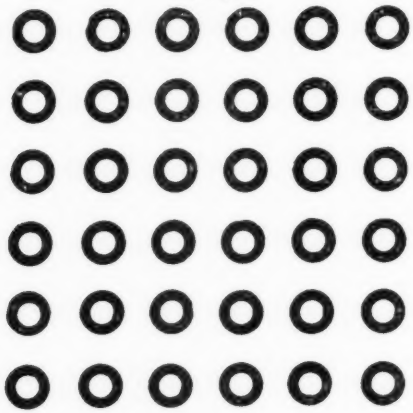
What, then, is the psychological point of view? The main thesis of the present paper is that the fundamental point of view in psychology is phenomenological. The psychologist, in common with all other men of science, takes as his starting point the acceptance of phenomena just as they are given. The experience relation is discovered within the order of phenomena and this furnishes to psychology its central problem. Every phenomenon *as it is given* is a function of bodily and more particularly of nervous conditions.

The necessity of a phenomenological foundation for the science of psychology is seen clearly in Fig. 2. Traditional structuralism has insisted that every datum in the science of

mind must be observed from a fixed point of view, and it has contended that a high degree of special training is necessary for psychological observation. I do not wish to discount the value for certain purposes of trained observation but merely to note that if psychology as a science restricts its scope to the systematic description of 'mental processes,' represented by the tone *W*, the great bulk of the experience relations represented in Fig. 2 and also the great bulk of phenomena ordinarily called 'mental' are thereby ignored.

Let it be frankly admitted that all phenomena, including the tone *W*, stand upon the same level of existence and that the experience relation applies to each and every one of them. This point of view is fully adequate to all problems which conceivably might arise regarding phenomena. It is not so much the *observation* of phenomena from a particular point of view as the *interpretation* of phenomena in a particular way that is fundamental to psychology as a special science.

FIG. 3



In order to demonstrate how the phenomenological point of view works out practically let me call your attention to the familar circles of Schumann (Fig. 3).[1] Assume that the circles are grouped by fours; they are observed in groups of

[1] Schumann, F., 'Beiträge zur Analyse der Gesichtswahrnehmungen.' *Zeit. f. Psychol.*, 1900, **23**, 1–32. Schumann's figure is slightly modified in Fig. 3.

four. Now assume that they are grouped by nines; they are observed in groups of nine. Now assume that they are grouped by sixes; they are so grouped. Various patterns are possible. The circles appear to be grouped as they are assumed to be.

Here, then, is a bit of raw experience. If we consider it as it is, at least two relatively independent phases are easily discernible. First, there is a configuration of blacks and whites. This organized structure is out there. It seems to be independent of the observer. When I move my head slightly and when I walk around the room it 'stays put' though the actual configuration changes more or less with my position. This phase of the experience depends upon my position in the room. It depends upon my eyes, for when I close them it is gone. It depends also upon physiological processes in my nervous system, but the exact nature of these bodily processes is largely hypothetical at the present time. Second, the grouping of the circles depends upon the assumption made. The assumption itself is not observed as a configuration. It may be stated verbally as a meaning.[1] The meaning or assumption, however, has a bodily basis. When I move about the room the assumption or point of view which determines the grouping travels with me. I believe the assumption to be represented by some sort of a bodily disposition—a nervous set,[2] perhaps, which is transported when I move. If my belief is correct, eventually it should be possible to interpret the meaning phase of experience in

[1] A fair amount of experimental evidence has yielded the result that meaning is not observed by the method of observation which reveals the sensory configurations. It would be fallacious to deny the existence of meaning on the basis of such evidence.

From the phenomenological point of view meanings exist. Significances come and go; they shift; they are found to be consistent and felt to be inconsistent with each other; they refer in significance to past and to future and to remote parts of the universe. The interplay of meanings is process-like; a particular meaning entering experience is an event; it is given at a definite time and it runs a definite course.

[2] The nervous system maintains 'postural sets' which are determinative. These may be regarded as the physiological equivalent of assumptions, beliefs, intentions, etc. For example, I determine to take a trip from Urbana to Madison. Presently I start and go. The conscious intention drops out of experience while I consider tickets, trains, and scenery. Presently I find myself in Madison.

physiological terms. Eventually it should be possible to interpret *every* phase of experience in bodily terms.[1]

It is encouraging to note that the *Gestalt* psychologists[2] are getting away from the restricted point of view of the traditional psychology and from the vicious postulates[3] underlying it and that they are beginning to consider experience as it actually exists. The phenomenological point of view seems to be coming to its rightful place.

If the phenomenological point of view be assumed as a working basis for experimentation, eventually, I believe, a genuine science of psychology will arise which will link together in an adequate way the facts of experience and the facts of animal movement. The linkage will not come through a confusion or a jumbling of the facts, nor will it come through an arbitrary blindness to them, but it will come on the side of interpretation through the exact study of the experience relation regarded phenomenologically.[4]

[1] It should be possible also to interpret physiological processes in terms of experience. At present the achievement of this goal seems remote.

[2] See the excellent presentation by: Koffka, K. 'Perception: An Introduction to the *Gestalt-Theorie*.' PSYCHOL. BULL., 1922, **19**, 531–586.

The following papers by Ogden are also in line with the writer's point of view: Ogden, R. M. 'Are There Any Sensations?' *Amer. J. Psychol.*, 1922, **33**, 247–54. "The Phenomenon of 'Meaning.'" *Amer. J. Psychol.*, 1923, **34**, 223–230.

[3] In the past psychologists have attempted to seek out mental elements called sensations, images, affections, etc. Today the doctrine of mental elements is under fire. The search for elements appears to be a *cul de sac* in a scientific maze. One reason is that the number of so-called mental elements is indeterminate.

[4] The writer is heavily indebted to the works of Avenarius, Mach, Stumpf, Külpe, and Titchener, and perhaps indirectly to Wundt. Reference should be made to the phenomenology of E. Husserl presented in his *Logische Untersuchungen*.

A NEW VIEW OF MENTAL DEVELOPMENT[1]

BY OLIVE B. GILCHRIST

The signal achievements of the past in the realm of physical science have been attained largely through close analytical study. A similar procedure has not unnaturally been followed in the newer mental and social sciences. Only by the reduction of the complex to simpler terms does any real understanding seem possible. As a sort of corollary to the proved serviceableness of this method, there has grown up the idea that the true unit of nature is the irreducible element, or elements, which, when once discovered, will permit of rebuilding the entire structure by a mere process of aggregation in accordance with natural law. This fundamental idea has been challenged by a group of contemporary psychologists who propose to replace the element by the pattern, or 'configuration' [*Gestalt*], as the genuine unit, both in physical and in mental science.

"The term *Gestalt* [pattern, configuration]," says Köhler, "is applied to those mental conditions and processes, whose characteristic attributes and effects are not producible by a summation of the corresponding attributes and effects of their so-called parts. . . . It is becoming increasingly clear that the fundamental and definitively characteristic form of the higher mental units is of the pattern type. Of late we find behavior of the patterned type already expressly singled out as the essentially distinguishing mark of the organic."[2]

Again, as opposed to 'the physical basis of consciousness,' "the demand is becoming insistent that organic behavior, which is held to correspond directly to higher mental behavior, must be conceived as sharing the essential attributes of the

[1] This paper was originally prepared for the Seminary on 'Problems in Mental and Physical Development' of the Harvard Graduate School of Education, 1923, R. M. Ogden, Editor.

[2] Köhler, Wolfgang, 'Die Physischen Gestalten in Ruhe and im Stationären Zustand,' Braunschweig, 1920, p. 9.

latter; organic processes must therefore be conceived as patterns [*Gestalten*]. This conclusion and its extraordinary significance were first clearly recognized by Wertheimer, who thereby attributed to the configuration idea a degree of reality far greater than it had hitherto possessed. More recently Koffka has ably championed the same view, and demanded with Wertheimer that the fundamental physical processes should not be regarded as sums of individual excitations—hence as mere chains of this added to that—but as patterned totalities." [1]

It is clear that this theory is consonant with the unity and continuity of instinctive action upon which Stout and others have insisted. "Instinctive action," says McDougall, "rather than reflex action, is, in my view, and in that of many other psychologists, the key to the understanding of human behavior. It is the teaching of this book that human behavior is built up on a basis of innate tendencies which are in all essentials very similar to the instinctive tendencies of animals." [2]

The ultimate test of any theory must be its fidelity to established facts in its field of application, and its adequacy in accounting for those facts. Let us therefore turn to a brief consideration of the infant and his development, especially in the light of this new interpretation. We shall then have some basis for judging of its merit as compared with other theories.

The study of the human infant is hedged about with so many difficulties that this important field of inquiry has suffered comparative neglect, even to the present time. There is, for instance, a not unnatural prejudice on the part of parents against having their babies used for experimental purposes. Moreover, it is difficult to hold a child under observation for a sufficiently long period to afford a satisfactory developmental study. The mother, who might seem to have both opportunity and peculiar insight for the study of her own child, is, in reality, too close to her baby to achieve the detachment essential to a scientific attitude, and is also

[1] *Ibid.*, p. xiv.

[2] 'Outline of Psychology,' New York, 1923, p. 70.

unqualified, save in very exceptional cases, to translate her intuitional understanding into scientific terms. The professional scientist, on the other hand, has limited opportunity, and is prone to read into his observation what he expects to find there.

Our present understanding of infant psychology is therefore largely derived from a very inadequate body of data based upon direct study of the child, supplemented by deductions from adult psychology and from animal psychology.

It is generally agreed that the child comes into the world with an inherited endowment of reflexes, instincts (or instinctive tendencies), and capacities, which are the prerequisites for his development by learning. He is, however, the most helpless of all newborn creatures, and there stretches before him a long period of dependence upon the care of others. Herein lies his opportunity. It is precisely these years which he can devote exclusively to growth, physical and mental, that determine his superiority over lower orders of life. The young bird, for instance, finds itself within a relatively short time possessed of its full equipment for life. The insect, whose brief day affords small leisure for development, comes yet more quickly to the fullness, and the limit, of its powers. So little does it learn, so slightly does one individual vary from another, that we go to the insect when we wish to study 'instinct' in its purest form. So quickly, on the contrary, does the infant begin to profit by experience, that infant psychology finds one of its most difficult problems in the attempt to distinguish what is innate, from what is acquired behavior.

While the original endowment of all children is alike in its elements, no two children are alike in the content, relative strength, and combination of these elements. In the heritage common to us all is rooted the uniformity of human nature, while in the special heritage of individuals, through direct ancestry or other little understood causes, lies the foundation of individual differences. The inherited disposition determines, to a large degree, both the possibilities and the tempo of growth. Development may be helped or hindered by a

multitude of environmental influences, but the bounds of maximum attainment are strictly limited by original endowment. Within this limitation the relative importance of heredity and acquisition is a question keenly debated by 'nativists,' on the one hand, and 'empiricists,' on the other, but without conclusive results.

Both the nature and the range of 'intelligence' are also variously conceived by different psychologists. According to the strictly mechanistic view there is, of course, no place for intelligence. It can have at best only a phenomenal existence, as a mechanically determined product of nervous activity. There has been in the past a disposition to set intelligence, as being chiefly a human attribute, over against instinct, as being the determinant of action in the lower orders of life. But this view is rapidly passing away, if it has not already been definitely abandoned. A seemingly more tenable conception is that expressed by McDougall, when he says that "Instinct and Intelligence represent neither two divergent lines of evolution nor two stages of evolution, but rather are always only two aspects of all mental life which we distinguish by an effort of abstraction," and that "the rudiments of mind are implied by the behavior of the most lowly animals." [1]

There is a close correlation between mental and physical development, whether from the individual, or from the evolutionary, standpoint, and there seems to be no doubt that a large degree of intelligence is conditioned by the cerebral cortex, or 'new brain.' The cerebrum is much larger in man than in the most intelligent animals, and it is noteworthy that the new-born infant seems to be more dependent upon it than any other species. Experiments made with a dog from which the cortex had been removed, proved that the animal was still capable of learning some things; on the other hand, a child born without a cortex never learned anything in its three years of life.[2] The superiority of the human cortex seems to consist, not merely in its greater bulk, but in its finer interrelation of parts, 'a closer knitting

[1] 'Outline of Psychology' pp. 202 and 203.

[2] Edinger, L., and Fischer, B., *Arch. f. d. ges. Physiol.*, 15, 2, 1913.

of the whole into a more complicated and more effective pattern.'

The behavior of the new-born child consists of cries and a considerable variety of apparently random movements of the limbs and of the eyes, the stretching of the body, etc. These movements are mostly of the reflex-type, and are also largely uncoördinated. There are, in addition, certain gross, wave-like movements of the whole body, which are more impulsive than reflexive or instinctive and which tend to disappear with time. The first instinct to assert itself is naturally that of hunger, and the act of suckling, which is fairly complex, is very early established. Like all human instinctive activity, however, it requires a certain amount of practice before perfection is attained.

Certain experiments with young dogs and rabbits showed that they possessed a lower degree of irritability than adults. The contraction and release of muscles was slower, the onset of fatigue more rapid, the capacity for inhibition less, and the threshold of response higher.[1] It is probable that lack of medullation and conductivity in the neurons may account, in part at least, for this slowness of response. It is also possible that perception in the infant is so dull that slight movements may not register in consciousness at all.

It seems clear that the earliest mental experience of the infant must be of the vaguest nature. He finds himself in a world that is quite new to him, and he has no categories to assist him in sorting the manifold stimuli that besiege his physical senses. The senses themselves are by no means the perfect instruments that serve him so efficiently in later life; and both motor mechanisms and instincts wait, in varying degree, upon the process of maturation for their full development. How, then, shall we conceive the beginnings of experience as they present themselves to the child?

We have seen that there are two quite divergent views of experience:

[1] Soltmann's experiments cited by Koffka, K. 'Die Grundlagen der Psychischen Entwicklung,' Osterwieck am Harz, 1921, p. 44 ff. Also English translation, 'The Growth of the Mind,' London, 1924.

1. The usual conception is that experience is gradually built up through a multitude of distinct reactions to discrete stimuli, received through the sensory organs from the outer world, or else originating within the organism itself.

2. The *Gestalt* psychologists claim that the configuration is the original and significant thing, both in the external and in the internal world. Koffka holds that the first conscious experience is that of a 'quality' against an undifferentiated ground. He says: "Not an unorganized ground, but a construction [*Struktur*] is the first object of perception; a quality against an undifferentiated ground. This view appears to me the more probable one. . . . A part of the world distinguishes itself from the rest; it appears as a quality; the rest, and it may in fact be very complex, appears at the same time as a uniform ground. . . . We must never proceed in such a way as to construe the phenomena corresponding to a given stimulus-situation, as though to every stimulus there were definitely attached that particular response which may, indeed, correspond to it under the conditions of a psychological experiment arranged with special reference to this stimulus. The assumption that a sensation is once and for all determined by the stimulus must be given up altogether." [1]

In support of their position the *Gestalt* psychologists call attention to various significant facts, for example:

1. The earliest behavior of the child does not indicate a great multiplicity of stimuli.

2. The child's first objects of recognition are of a very complex nature, as the human voice and the human face. The child also learns to distinguish a smiling face from an unfriendly face long before he shows signs of discrimination between simpler objects. He seems to display some rudimentary power of abstraction, whereby he arrives at an understanding of details through the pattern or configuration of the experience, but not vice versa.

3. In later life, weariness or illness often tends to produce a dull monotony of perception, in which the general pattern

[1] Koffka, K., *Ibid.*, p. 97 ff.

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remains, although the details drop out of attention. This would seem to be a sort of reversion to an earlier type of experience.

A considerable body of experimental evidence is available which adds weight to these considerations. It has been proved that both hens and apes can be trained to select food-boxes through relative rather than actual color.[1] Having become accustomed to find their food in the lighter colored of two boxes, hens will still select the lighter box even when the colors are changed. Such a selection is obviously not explainable on the basis of an absolute reaction to an absolute discrete stimulus.

Among all the problems of mental development, there is none more difficult than that of learning, *i.e.* the fundamental question of how the child, or the animal, ever learns anything. Learning is based upon memory, and in the days when memory was held to be one of the special 'faculties' of the mind the question appeared simpler. One object or one idea was supposed to call up another by the law of association. But as science became more critical it seemed necessary to inquire how 'association' worked, and why a given stimulus called back one thing in the past, rather than many others with which it had also been associated.

With the rise of the mechanistic psychology, it became necessary for its proponents to seek the explanation of this, as of all other mental phenomena, in the structure and functioning of the nervous system. The association of ideas now took on a purely concrete aspect as a matter of intricately connected neurons. When two objects or ideas had once been brought together in experience, a 'nervous pathway' was said to have been opened along which an impulse was likely to travel in the future, thus permitting the 'recall' of one idea by the other. The law of the 'conditioned reflex' (based largely upon Pawlow's experiments with dogs) states that any stimulus may become 'adequate' to call forth any response through association with the stimulus originally

[1] Köhler W., 'Nachweis einfacher Strukturfunktionen beim Schimpansen und beim Haushuhn,' *Abh. d. Preuss. Ak. d. Wissenschaft. Phys.-Math. Kl.*, 1918, No. 2.

adequate to evoke that response. Given this law, with a limited number of congenital reflex-arcs to initiate experience, the problem seemed to many mechanists to be well on the way to solution.

But there still remained the question of choice and habit formation, *i.e.* the questions why *certain* nervous pathways were chosen and habitually used rather than others, for which connections also existed. Watson, after considerable experimentation with white rats in a maze, came to the conclusion that two simple mechanical laws were sufficient to explain the fixation of habit, viz. frequency and recency of repetition.

Thorndike felt the need of a more specific explanation of the choice of certain responses to the exclusion of others. Accordingly he performed his now famous experiments with hungry cats in a cage from which they could escape by a puzzling device, and thus obtain food displayed outside. As a result of this study he reached the conclusion that it was the association of satisfaction with a given response which caused it to persist, while dissatisfaction (failure) connected with a response caused it to be abandoned. Thus random movements which failed to bring success were gradually eliminated, and the cat 'learned' in time to go directly to the goal. Thorndike formulated his findings under the 'Law of Exercise' and the 'Law of Effect.' The first, subdivided into the 'Law of Use' and the 'Law of Disuse,' covers essentially Watson's position, while the second embodies his own idea of selection on a basis of satisfaction and dissatisfaction. Learning thus becomes a process of forming 'favored pathways' in the neurons, through the operation of 'satisfiers' and 'annoyers,' *i.e.* situations which are attended by success and failure, respectively, in leading to a satisfaction of wants.

Lloyd Morgan's theory is akin to Thorndike's, though not identical with it. He holds that 'serviceability' is the determining factor in habit formation. 'Instinctive behavior, as congenitally determined,' he says, 'is practically serviceable on the occasion of its first performance.'[1] On this basis of serviceability, according to Morgan, certain trains of action

[1] 'Instinct and Experience,' New York, 1912, p. 22.

are gradually built into behavior, while for the lack of it others are eliminated.

Stout offers a quite different theory of learning, corresponding to his belief that instinctive activity is characterized by 'an awareness of the present situation as transitional' [1] until something vaguely expected is attained. "The unity and continuity of interest which binds a sequence of distinct acts into a single action has its counterpart on the side of retentiveness in the formation of a cumulative disposition. On the first occurrence of the process the traces left by prior phases persist in and contribute to determine succeeding phases. They unite in a single cumulative disposition. When the activity is repeated, whatever stimulus prompts it reëxcites the total cumulative disposition left behind by its previous occurrence. The cumulative disposition has been modified in the anterior experience, and accordingly the rearoused activity takes a correspondingly modified course as a whole. This is the process which we have described as *revival of acquired meaning*. Without this there can be no learning by past experience; and intelligent learning by experience may be due to it alone." [2] It is clear that we have here something approaching the view of the *Gestalt* psychologists.

There has been a growing feeling on the part of investigators that experiments of the maze and puzzle-box type were ill-adapted to furnish data from which to deduce a theory of learning. What we wish to know is not how a child or lower animal stumbles upon, and finally masters, the solution of a riddle, by the trial and error method, but rather how in circumstances calculated to give fair play to innate capacities of whatever sort, the process of learning is carried through in dealing with the practical problems of life. It is not easy to devise tests for adults which will throw light on this process in its elementary form, so thoroughly is all adult procedure molded by artificial methods. The same holds true, in lesser degree, of the child, whose plasticity renders it difficult, even at an early age, to draw the line between that which is innate

[1] 'Manual of Psychology,' 3d Ed., London, 1913. p. 355.

[2] *Ibid.*, p. 384.

and that which is acquired. In order to attack the problem at the point where intelligence is considerable, while instinctive activity remains relatively less mobile, it is necessary to go to the animals lower than man. This various investigators have done in recent years, with deliberate intent to substitute a more natural task for the mere riddle or puzzle. Among the studies of this type none has been more significant than that of Wolfgang Köhler with chimpanzees on the Island of Teneriffe.[1]

Köhler presented to his nine apes problems of securing food, which ranged in difficulty from those which were easily solved by all to those which were handled by none with full success. They were so planned as to give the apes an opportunity to exercise whatever insight they might possess. The tests included the catching of a swinging basket, in order to reach which the ape had to climb up the side of the cage; the use of a stick to snare fruit placed outside of the cage; the securing of a longer stick outside the cage, by the use of one inside not itself long enough to reach the basket; the fitting of two sticks together to secure length adequate to accomplish the task, etc. Other variations involved the removal of a box obstructing the way to the food; the removal of stones from the same box; the securing of a limb from a dead tree with which to draw the fruit to them; and the loosening of a rope wound around a bar in order that it might be used as a swing to reach a suspended basket. The apes found the last two tests hard to master. It appeared difficult to perceive the limb of a tree as a separate thing which might be detached and employed as a stick. It was also hard for the ape to uncoil a rope. Both these difficulties seem to be perceptual in nature. Still another test, which required the piling of one box upon another in order that the ape might reach a basket suspended from the ceiling, was accomplished, but never with any attempt to make the structure stable. Again a perceptual deficiency is apparent; for the ape never learned to pile the boxes evenly, but only to extend the structure upwards in the direction of the goal.

[1] 'Intelligenzprüfungen an Menschenaffen,' Berlin, 1921.

It is also noteworthy that while Köhler's apes showed great variation in intelligence, he found in all his experiments only eight of the 'stupid errors' upon which Thorndike bases, in part, his denial of insight to animals. There were, on the other hand, many 'clever errors' which were no less significant than the successes, *e.g.* the attempted use of two sticks by pushing one with the other until contact was made with the fruit basket. Although this contact was of no avail in obtaining the fruit, a certain degree of insight seems to be indicated by this error.

Köhler's conclusions were quite contrary to those of Watson and Thorndike. Instead of attributing learning to chance solutions, he found this type of behavior readily distinguishable from a true solution, which, far from being the product of frequency or recency of repetitions was carried out the first time it was tried with the same suddenness and sureness displayed by a human being when he 'sees through' a thing. Once achieved, a solution of this sort was never lost. Nor was there any occasion to invoke a 'Law of Effect.' In certain cases, at least, it appeared that success followed as the direct result of a careful consideration of all the circumstances. Koffka sums up the results as follows: "We have found that the animals mastered, by a genuine solution, new problems with which they were confronted for the first time; and we have seen that the essential characteristic of these solutions did *not* consist in movements already habitual to the animals, passing over into new combinations, but consisted rather in a 're-patterning [*Neu-Strukturierung*] of the whole field.' The first hypothesis could be maintained only if one were disposed to regard chance as the creator of these new combinations. That chance can have played this rôle in the experiments of Köhler is an impossible hypothesis to anyone who has understood the experiments.[1] . . . We meet here again that principle of self-contained unity [*Geschlossenheit*] which we have found before. In the solved situation everything, every movement, has its own place as determined by the total configuration; the configuration has become fixed and

[1] 'Grundlagen,' p. 144.

complete in its unity of meaning. As a dynamic patterned activity [*Struktur*] going forward in time—we understand now by pattern not only the field of perception, but the whole process of solution to the attainment of the goal—it has a beginning and an end." [1]

Köhler's experiments have been discussed, and his conclusions questioned, by several German psychologists. Karl Bühler would recognize a consciousness of *relationship* on the part of apes, but not of *insight*. Lindworsky would acknowledge only the working of instinct, where Köhler persists in finding intelligence. The important question seems, however, whether or not the 'configuration' idea, so clearly expressed by Koffka above, offers a better explanation of the facts of experience than the older mechanistic theories.

If we consider the experiments of Watson and Thorndike, we realize that they were planned on the definite assumption that animals are machines, and with a view to proving them such. But does not this assumption vitiate their conclusions? Barring Thorndike's Law of Effect, the mechanists must fall back upon a virtual association of ideas (translated into terms of neurons and synapses); and we have already seen the difficulty of accounting for any selective process on this basis. Köhler's findings tend to disqualify both the 'Law of Exercise' and the 'Law of Effect.' But the latter is weak at best; for if success makes possible the stamping in, or retention of a response that precedes it, this effect must be produced by a retroactive power, and for such a reversal of action in the neurons there seems to be no explanation.

Even granting, however, the validity of the Law of Effect, the mechanistic theory fails to account for the highly adaptive character of instinctive activity. For instance, it is difficult to conceive any purely mechanical chain of reflexes which would produce the active seeking and the direct adaptation to varied situations that belong to the nest building activity of birds. Still less has mechanism any explanation of such an extraordinary break from the reflex-chain as that represented by the wasp's using a stone to pound down the earth above her egg.

[2] *Ibid.*, p. 146.

Finally, the mechanistic theory is unsatisfactory (1) in that it either reduces consciousness to illusion or refuses to accept any responsibility for explaining it, and (2) in that it nullifies irrevocably not only conscious purpose, but all the higher ethical and emotional values that give significance to life.

If we turn now to the configuration theory, we find it highly suggestive where the mechanistic theory is weakest. We have seen that, independent of the *Gestalt* psychology, men like Stout and McDougall have evolved hypotheses which are closely akin to it. They have detected the essential unity of behavior, and the striving forward to a goal, dimly felt if not perceived, which are so characteristic of instinctive activity. They have seen that an end to be attained seems to shape the course of instinctive action, thereby imparting to it a continuity in spite of widely varying details. From this conception to that of the configuration, in which the whole stimulus-response series is linked into one dynamic functional unity, is not a very long step. Through the whole configuration runs a rhythm, a meaning which is incomplete until the last step is reached. It is this that gives to behavior the character of *Geschlossenheit* which marks the *Gestalt* as the true unit of experience.

Recognizing an obvious connection between mental life and the physical organism, Koffka and others assume that consciousness has a neurological correlate, wherein pattern answers to pattern, rather than point to point in a discrete series. While this appears to be a large assumption, it is necessary to postulate something to bridge the gap between the external and the internal, between stimulus and response; and in our present ignorance of the actual nature of the existing bond, rival assumptions can be evaluated only in the light of known facts, and by their seeming helpfulness in interpreting the same.

Tested on this basis the new theory appears at least more tenable than the old. We have already seen that the earliest perceptions are of the pattern-type. Experiment has proved that it is almost, if not quite, impossible to learn anything

that is devoid of significance. The memorizing of nonsense syllables seems to be dependent upon some organization of rhythm, if nothing more.[1] In other words, the 'pattern' appears to be an essential factor in any reaction-series which is to be recalled, and memory reproduces it in its patterned form. Numerous minor variations may occur in the series, but the unity of meaning is retained in the total configuration.

The new conception is suggestive also in its application to the vexed question of imitation. We lack at present conclusive evidence to prove that the child either does, or does not, learn new things by imitating others. It is, however, unquestionably true that imitation aids materially in the acquiring of facility, and that imitation increases with enlarging comprehension. It would therefore seem easier to explain imitation in terms of dynamic patterns, each with its own internal organization and meaning, than in terms of chain-reflexes, in which each link must be produced individually with the preceding link as a stimulus.

If, finally, we consider the *Gestalt* theory in its broader implications, it appears to possess distinct advantages over that of the mechanists. It recognizes consciousness, not only as a real datum of mental life, but as the essential field of psychology. Philosophically it suggests a middle ground between vitalism and mechanism. It will bear either a parallelistic or an interactionist interpretation. On the other hand, it does not necessarily lead to a spiritualistic view of life. It is, in short, so far as we can see at the present time, free from all limitations which would condition its validity upon one plilosophical position rather than upon another.

To sum up in a few words the conclusion to which we are led by our consideration of the mechanistic *vs.* the *Gestalt* theory, we may say that whereas the former seems fundamentally untenable, the latter, though presenting difficulties, does no visible violence to known facts, and, furthermore, seems to offer a possible means for their interpretation.

[1] McDougall, W., 'Outline of Psychology' Note p. 303.

XUM

NOTES ON 'FALSE' REACTIONS

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In reaction experiments, one is commonly asked to make a conventionalized response in a standard situation, such as raising the right hand from a telegraph key on seeing a white card exposed. The response called for becomes, conventionally, the correct response. Sometimes, through inadvertence, response different from that called for is made, as in the case above, raising the left hand instead of the right. Such processes may be termed 'false' reactions. They represent a breakdown of the subject's adjustment to the experimental situation. He does something which he normally 'knows better' than to do.

In these terms of course, one can describe a great many kinds of maladjusted reaction in practical life from a mere slip of the tongue to the disorganization of a psychosis. All are 'false reactions' more or less continuous and systematized. The question arose as to whether maladjustments of this deeper level were reflected in proneness to false reactions on the more superficial level of laboratory conventions. A series of experiments was carried out in the laboratory of McLean Hospital,[1] in which comparison was made in the liability to false reactions of a number of normal individuals and psychopathic conditions. No significant difference was found. In numbers of false reactions produced, the extremes in the normal individuals varied as 0.6 to 5.0; the pathological cases as 0.6 to 4.6. The average would be in favor of the pathological conditions. Individuals of very distorted instinct trends and thought processes, did unexceptionable work in the experiments. The most false reactions were made by a man of excellent personality and of national distinction

[1] Wells, F. L., and Sturges, H. A., 'The Pathology of Choice Reactions,' *Amer. J. Insanity*, 65, 81–119.

in his profession. The design of these particular experiments at least, was not suited to illustrate differences in aptitude with fundamental adjustment problems.

Practical life furnishes evidences that superior abilities in complicated motor adjustments do not carry over into the realm of social adaptations. No accomplishment demands higher motor skill than instrumental music; but the genius thereof is regularly applauded for this accomplishment rather than for clearly social ones. Our age has one figure preëminent in the concert of nations as well as of music, Paderewski, but his accomplishment is hardly a typical one.

A study by Henmon [1] afforded a numerous material for the study of false reactions, though with a limited number of subjects and a rather specialized class of stimulus. Henmon's studies employed two techniques, distinguished from the present standpoint as follows: In the first case, the stimuli to be differentiated were easily distinguishable, as in the shades of red and orange, or the various pitch differences used. In later experiments [2] lines were employed, differing in length so little that they could not always be correctly distinguished. If then there was a false perception of the stimulus, the reaction would be wrong, and from an objective standpoint, false. It would, however, be in accord with the stimulus as perceived, which would hardly be the case with such contrasting stimuli as red and orange, or black and white.

Wrong reactions of this kind are found to increase in number as the differences in the stimuli become smaller. At the same time the perceptions become less clear, as indicated by the lesser degrees of confidence in the judgments. In general, it did not appear that wrong judgments, as such, took more or less time to make than right ones. But on closer analysis, it appeared that there were two kinds of wrong judgments; one being quasi-premature reactions, the other being a group of reactions 'prolonged beyond the optimal time.'

These latter fall into line with observations recently re-

[1] Henmon, V. A. C., 'The Time of Perception as a Measure of Differences in Sensation,' *Arch. Phil., Psychol. and Sci. Methods*, 1906, No. 8.

[2] Henmon, V. A. C., 'The Relation of the Time of a Judgement to its Accuracy,' PSYCHOL. REV., 18, 186–201.

ported by Dr. Grace E. Bird on 'The Devious Path of Slow Work.'[1] In connection with generally positive correlations observed between rapidity and accuracy of work, the point is made that delay in the completion of a mental process favors the formation of 'irrelevant bonds' and a more devious means of arriving at a solution. From this it is a short step to the idea that anything tending to delay the process will favor the entrance of irrelevant factors tending to distort the process. It has already appeared that the more closely the difference of liminal stimuli approaches zero, the more 'wrong' reactions are made, involving misperception of the stimulus. And the same is true of 'false' reactions, to differences clearly above the threshold. As the differences become smaller, the time of both correct and false reactions is increased. And with this increase in reaction time, comes an increase in the number of false reactions. Not only is the path of slow work devious, but it more often leads astray. Indeed, in Henmon's observations and those of the writer to be quoted, false reactions are not generally distinguished by longer times than correct ones in corresponding circumstances. It is simply that any circumstances tending to make the correct reaction long, will also increase the likelihood of false reactions of corresponding length.

To this general statement one reservation appears. In the study by Wells and Sturges above mentioned, there were two reaction series, known as 20, and 10, involving relatively simple psychomotor processes. For normal subjects the reaction time scores averaged 840 and 870 sigma, the numbers of false reactions 1.2 and 1.3 per series respectively. A more complicated series termed 50 had a comparable time score of 1,410 and averaged 4.1 false reactions per series. This is in accord with the above. But there were also two series involving equally or more simple motor adjustments but relatively high level mental ones. These were denoted as 110 and 100. In these series the times were considerably longer than in series 20, and 10, 1,180 and 1,350 sigma respectively. On the other hand, they produced slightly fewer

[1] Bird, Grace E., 'The Devious Path of Slow Work,' PSYCHOL. BULL., **19**, 78–79

false reactions than series 20 or 10. This appears contradictory to the above. The explanation seems to be that series 50 was from a mental standpoint simple, but involved the establishment of several novel motor habits. Series 110 and 100 were mentally of a higher level, but their motor patterns are relatively simple and natural ones. Series 50 may be compared to a road which is short, but in very hazardous condition, where the incautious driver is promptly bumped into the ditch. Series 100 and 110 represent roads in excellent condition, but so much longer that they take nearly the same time to traverse as the shorter and rougher one. Thus, a delay due mainly to the length of the arc, is not specially productive of false reactions, provided the arc be an unobstructed one. It is delays due to mental intersections, grade-crossings and 'thank-you-marms,' which are more likely to lead the reaction to a false destination.

The main concern in the study of Wells and Sturges was individual differences in tendency to false reaction. The question was if psychotic conditions would show more such tendency than normal individuals, and such a situation was not found. In the work of Henmon, however, as well as in the later study, there seemed to be marked individual differences, and the question of their interpretation remained an open one. In the present writer's work on these results, no criterion of individual difference was employed other than the global tendency to false reactions in a number of mental processes. Certain individuals were found to show more false reactions than others, and certain types of process were found to elicit more false reactions than others. The question was not raised of significant individual differences in the types of mental process eliciting false reactions. A suggestion of this kind might have been afforded by the fact that while Series 10, 20, and 50 of Wells and Sturges did not differentiate normal and psychotic conditions in reaction time, Series 110 did so relatively well, and Series 100 quite well. That this distinction should be made in respect to speed and not in respect to false reaction is noteworthy. The major psychotic difficulties are, of course, in doing the wrong thing, not in doing the right thing too slowly.

This raises the question whether speed of reaction and tendency to false reaction are related; whether a rapid reactor also makes more errors, etc. That at the higher levels of practise both speed and accuracy are greatly increased, is commonplace. But the majority of life situations are not those in which one has acquired a great amount of special practise; they are in this respect more comparable to the practise that obtains with the ordinary, more or less casual subject in psychological experiments. Under these conditions, the practise of Henmon's experiments did not even clearly affect the amount of individual difference shown. Moreover, when one thinks of the combined speed and accuracy of the skilful typist or pianist, it must be remembered that they represent selected groups, those specially trainable in these functions. It does not follow that the population at large would show or develop such combinations.

The experience to date is small for generalization, but at all events it is against making generalizations for processes of the levels observed. There seem to be all grades between the fast and accurate reactor, the fast and inaccurate, the slow and accurate, the slow and inaccurate. In Henmon's experiments, Subject I was in all functions observed faster and more accurate than the other two subjects. In Series 50 of Wells and Sturges' experiments, there was moderate negative relationship between speed of the process and the numbers of false reactions.

In both speed and accuracy, Henmon's subject I was superior to II with respect to discriminating differences of pitch. The question was raised as to how such superior discriminativeness would appear in a right and wrong cases experiment. Such an observation was made, and showed a lower threshold for II combined with a shorter simple reaction time in this subject. Evidently the factors producing lower speed and more false reactions in II were something rather specific to the choice reaction situation. Attention has been more recently invited to the differences in reacting to a similar situation according to whether the

reaction is with the hand or with the voice.[1] From the standpoint of the behaviorist, it is interesting that the solutions of quite similar mental problems should be so affected by the character of the motor response that is to be made.

Additional light is thrown on some of these points by the observations that are the precipitating cause of the present remarks. In certain experiments already described [1] record of false reactions was kept. There were 204 of these among five subjects, about three percent of all reactions observed, five percent of all choice reactions. The choice reactions were all concerned with making a differential reaction to black and white. But in Series 1 and 2 the exposure was with a Dodge mirror tachistoscope, reaction being with telegraph keys. In Series 3 and 4 and the remainder, exposure was with a Dodge spring pendulum exposure apparatus. In Series 3 and 4 reaction was again with the telegraph keys. In Series 5 and 6, the subject named the side on which white or black was exposed, instead of raising the key on that side. In Series 7 and 8, the subject named the shade exposed on the given side.

Some of these processes differed characteristically in their susceptibility to false reactions. With the mirror apparatus, there were eighteen false reactions to white. There were forty false reactions to black. Every subject but one had over twice as many false reactions to black as to white. In effect there was thus a tendency to raise the hand on the side on which white appeared.

When the same instructions were given but the exposure made with the pendulum apparatus, there were forty false reactions to white and but eleven to black, the reverse of the former situation. This is not to be interpreted as an inherent difference between the two shades in calling out false reactions, or a different tendency to them in the right and left hands. The essential difference seems to be in the character of the exposure. In the mirror apparatus, the background is black, and harmonizes with the black of the stimulus. In the

[1] Wells, F. L. 'Vocal and Manual Mechanisms in Choice Reactions,' J. Exper. Psychol. 7, 59–66.

pendulum apparatus, the background is white; it is undifferentiated from the white stimulus proper, while the black contrasts with it. In both cases, therefore, the most false reactions occur on the side with the more contrasting shade. It illustrates, on a primitive level, a rather far-reaching principle of human conduct. There is an impulse to positive reaction towards that which attracts attention, which may overcome a conventional setting against it. The expression attributed to P. T. Barnum, that it matters little how you become known, the main thing is to get your name before the public, voices the same idea at a more complicated level.

Series 5 shows a total of 39 false reactions and Series 6, 36. The factors which made the differences in the previous series do not obtain here, for each series contains an equal number of reactions to black and white. This is true also of Series 7 and 8. But Series 7 and 8 do not produce a third as many reactions as do Series 5 and 6. The false reactions in them number 9 and 11 respectively. To distinguish black and white according to 5 and 6, is evidently much more confusing than to do so according to Series 7 and 8; it also takes longer to do it correctly, 91 sigma longer on average. There is, of course, no difficulty in perceiving the side on which a given shade is exposed. The trouble comes in getting the reaction into the requisite form. It presents a problem somewhat analagous to Series 50, of the Wells and Sturges experiments, which were also prolific in false reactions. In the present case, the analogy would be rather a network of roadways generally well travelled, but among which it is difficult for the desired reaction to find its way. Series 7 and 8, on the other hand, follow a straight and easy road, that of simply naming the shade.

In the present results there is a well-marked tendency for the false reactions to be shorter than the correct ones. In general this difference is not so great as to stamp the false reactions as typically premature. In one subject this seems to be the case. Here the false reactions in the various series run from 25 to 44 percent of the correct ones. Most, if not all, this subject's false reactions must have been premature.

In numbers they show nothing distinctive; neither does the length of the correct reaction times. In its broader relationships, this tendency to premature rather than delayed false reactions is, for this case, associated with the generally more valued traits of personality.

The three most conspicuous cases of delayed false reactions occur two with Subject V and one with Subject I. In Subject I, nine false reactions, averaging 113 percent of the correct time, are reactions to black when they should have been to white, in the pendulum apparatus. In Subject V, eight false reactions, averaging 118 percent of the correct time, appear in Series 5, and six false reactions, averaging 123 percent of the correct time, appear in Series 6. These series were of outstanding difficulty for Subject V. They are much the longest reactions in the whole range of the experiments, and disproportionately long in relation to the other reactions of this subject. But in the experiments as a whole, only one subject made fewer false reactions than Subject V; and Subject V, with fourteen false reactions in Series 5 and 6, was the only subject to make none in Series 7 and 8. The exceptional difficulty of the subject with this process of 5 and 6 seems to be an idiosyncrasy such as have been seen from time to time in the writer's other experience with reaction time work.

The premature reaction, as notably illustrated by Subject III, expresses initial over-tension in the reaction system, which is set off by the stimulus without differentiation, in a reaction with one or both hands (where the reaction is manual). Delayed false reactions express a disorganization of the reaction system; the sought for response is blocked, and this produces a rise of tension which is broken through blindly to bring it to a close. In the majority of false reactions both these factors are probably present. The resultant is a false reaction time not characteristically longer or shorter than correct times, but much more variable. To these must be added such special factors productive of false reactions as are inherent in the stimulus itself. The observed tendency of white on black ground, and black on a white

ground, to draw the reaction to itself, is an example of this.

No consistent relation is shown between general length of choice reaction time and absolute number of false reactions. There are extremes and gradations between them. The subject with much the most choice reactions has also the next to the longest general time, but this is the only case of the kind. The general average choice times vary from 306 to 394. Subject IV has the shortest general time, combined with the median number 37, of false reactions. Subject I has the median general time score, 356 sigma, and the smallest number of false reactions, 26 for nine experimental series (the others had ten).

The study of false reactions is one of much theoretical interest beset with equal practical difficulties. They are essentially by-products. By definition, they are gathered from experiments made for other purposes. When artificial conditions, such as distractors, are introduced into the experiment to increase the number of false reactions, their character as strictly 'false' reactions is vitiated. It would be the same if one should make the false reactions the topics of special introspection, or in any way draw unusual attention to the subject to the false reactions. They would lose their false character, and become integral parts of the experiment.

Modern psychometrics have made the organization of the intellectual processes as such, the best understood aspect of the personality. In their general relation to personality study, false reactions are little concerned with this. They deal more directly with making mental processes effective in action, and so with the study of that dislocation of noöpsyche and 'ergopsyche,' which shows increasing influence on psychometric studies. Some suggestions of Henmon's experiments were carried out with negative results in the work of Wells and Sturges. The present observations bring to light complexities in the problem not fully appreciated by the writer, at least, at the time of the earlier work. Considering general tendencies to false reaction does not lead far. But the beginnings of analyzing the problem are made. One distinct field of inquiry is the meaning of premature as com-

pared to delayed false reactions, similar to the two types of 'wrong' judgments observed by Henmon. Probably these have more significant meanings for the personality in general. Another topic concerns the various types of mental process by which false reactions are elicited. These appear to differ with different persons, presumably also with further meanings for the general mental constitution.

A THEORY OF LEARNING

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The purpose of this paper is to set forth a theory of brain action that will explain the various facts of the psychology of learning. It is necessary first to see just what the psychological facts are.

Learning is a process of connecting. In habit-formation we are establishing connections between responses and their stimuli. In knowledge-getting we are establishing connections between ideas.

The first thing to be noticed about these connections is that they are either between the parts of simultaneous experience, or between one experience and another which immediately follows it. It is possible that connections are also established between experiences that are not temporally adjacent but are separated by a short interval of time. The following may serve as an illustration of a connection established between the parts of a simultaneous experience: I see a man and hear his name spoken at the same time. As a result there is a connection established in my mind between the image of the man and his name. The image of the man and the representation of his name may take various forms. Connections between successive experiences may be illustrated by piano playing. The stimulus is the perception of notes in certain positions on the musical scale. The response is the striking of certain keys on the piano. Learning to play the piano is the process of establishing and fixing the connections between the visual perception and the motor response. After practice, the idea of the note and its position may take the place of perception of the note.

The second psychological fact has reference to the strength of connections. Not only do the parts of experiences, simultaneous or successive, become connected, but there is a differ-

ence in the strength of connections. Some connections are much stronger than others. Connections formed between processes of attentive consciousness are stronger than connections between processes of inattentive consciousness.

Consciousness ordinarily has a high attentive level and a low attentive level, *i.e.* a group of clear processes and a group of vague processes. In some cases there may be more than two levels of clearness. The connections established between the processes of clear consciousness are relatively strong; while those established between the processes of vague consciousness are relatively weak. Two cases may be noticed: (1) A bond that is formed between clear processes of consciousness is stronger than a bond that is formed at the same time between processes that are vague. In fact, connections formed between the vague processes usually immediately disappear. It may be even nearer the truth to say that such connections are not formed. (2) Connections formed between simultaneous or successive processes that are clear may vary in strength in accordance with the degree of clearness. The clearer the processes, the higher the level of attention, the stronger are the connections that are formed.

A third fact concerns the difference between good and poor learners. People who are good learners have what is commonly called a *high power of concentration*. In more scientific phraseology, their conscious processes have a focus clear and well-defined as compared to the simultaneous vague processes. Poor learners are poor in attention. Their conscious processes are not clear-cut; the difference between the clear and the vague is not so marked. Good learners are able to stick to a task and maintain a continuously high level of attention. They have a continued succession of clear processes grouped about a central thought or idea. Poor learners vacillate; they cannot stick to a task long at a time. The consciousness of the poor learner differs from that of the good learner both transversely and longitudinally. On the one hand, it does not have the marked difference in clearness-value between what may be called the high level and the low level of consciousness. On the other hand, there is not the

continued succession of clear processes related to the same idea as is the case with the good learner. In plain words, the poor learner does not work so long at the same task as does the good learner nor does he work so effectively while he does work.

Another difference between good and poor learners is that the former see what is significant. The important, meaningful aspects of their experience is apparent to them. The world in general is to them a more meaningful world, and each individual experience is to them richer in meaning than is the case with the poor learner. Aspects and attributes of objects appear to the good learner that do not appear to the poor learner. To illustrate: in sorting cards into separate boxes, good learners discover devices of associating or fixing the location of different numbers that never appear to poor learners. So, also, in learning a series of nonsense syllables, the good learner makes a meaningful series out of what remains meaningless to the poor learner. In studying a book in the ordinary process of preparing lessons, the good learners look for meanings and find them, while poor learners get little or nothing from the study.

It has been often said that genius is the ability to see similarity in different experiences. Now, similarity of experience is in reality *identity* of experience. To the good learner subtle identities in different experiences appear that are entirely unnoticed by the poor learner. Newton identified the rate of acceleration of the falling moon and the rate of acceleration of the falling apple as due to the same cause.

Such, in brief, are the main psychological facts as they appear to observation and experiment. We must look to the brain for an explanation of the facts,—to brain anatomy and physiology. The anatomy of the brain is fairly well known, but the brain physiologist cannot yet answer all the questions which the student of the learning process would like to have answered. We must resort to theory. We must picture a form of brain action that will harmonize with the observed facts. We do this in the hope that our picture, our theory, may guide or at least stimulate investigation.

Most of the psychological facts concerning the learning process center in one way or another about attention. An acceptable physiological theory of learning must therefore be in harmony with the facts of attention. The physiological correlate of attention is cerebral focalization. The brain is a unifier. The sensory stimulations constantly excite it to activity, but there is always a synthesis of this activity. A process of unification is always going on. Unification is accomplished in this way: a few processes become focal, monopolizing the brain activity for the time, while the other simultaneous processes are wholly or partially inhibited. That is to say, they proceed on a lower level of activity or are suppressed altogether. One of the chief functions of the brain seems to be to harmonize response and stimulus, *i.e.* to unify action. We cannot respond at the same time to all the stimulations of the moment. Natural selection has developed in man a brain in which there is always a selective synthesis of activity, a few processes becoming focal, and monopolizing for the moment the function of initiating action. All other simultaneous cerebral activities are wholly or partially inhibited. Three laws of brain action can be formulated that will account for all the facts in the case of bonds of simultaneous experience. (1) Simultaneous brain activities are connected. (2) Only in focalized brain activities are strong connections formed. (3) The more intense and vigorous the focalized brain action, the stronger are the associative bonds that are formed.

The brain is a theater of constantly contending influences. In it a resultant is constantly established. It is as if there were an influence always working toward focalization. This influence is positive. Another influence always works toward suppression or inhibition. This influence may be considered negative. Although our brain is a composite, although there are visual centers, auditory centers, smell centers, etc., the brain seems always to work more or less as a whole. It does this by facilitating, focalizing a few activities, and inhibiting all other simultaneous activities. It is as if the brain had only a certain, definite amount of energy, and at any one moment uses this energy in a restricted direction or field.

XUM

The excitations that constitute brain activity may be thought of as if they resembled electric currents. Now, an electric current of a given pressure or potential will be relatively weak if it pass through many conductors. That is to say, if a current is divided and passes through several conductors, it will be relatively weak in all of them. The same thing seems to be true of neural currents. A neural current may be thought of as having a certain potential or pressure. If it is diffused by passing through several routes it will be everywhere weak. If its passage is restricted to one nerve path or to a very few, it will be relatively strong. Some people seem to have high-pressure brains; others, low-pressure brains. In high-pressure brains, the difference between the focalized and non-focalized activities is great. In low-pressure brains, there is not much difference between the focalized and non-focalized activities. The high-pressure brain forms strong and persistent associations between different activities. People having high-pressure brains are good learners; those having low-pressure brains are poor learners. To the person having a low-pressure brain, experiences are much alike. There is not to him the clear-cut difference in the importance of things which the person of high-pressure brain always sees.

To return now to the three laws of brain action; strong bonds are formed between simultaneous brain activities. This is not theory but a statement of fact, though our explanation of the reason why such bonds should be formed must be theoretical. Let us take the case of two simultaneous focal brain activities, and let us suppose that one of these activities is in neuron *X*, and the other in neuron *Y*. Excitation in *X* passes out along the axon of the neuron to all parts of the brain, most where there is the least resistance and least where there is most resistance. The same thing is true concerning the excitation in *Y*. Now we have to make only one postulate to be able to explain why *X* becomes coupled with *Y*.

The postulate is the following. The dendrites of a cell in action make good connections with the brush ends that touch them. The more violent the neural activity, the better is the connection. It therefore follows that the excitation

going out from *X* finds *Y* most open. The excitation going out from *Y* finds *X* most open. *X* flows into *Y* and *Y* flows into *X*. A pattern of brain action is formed, the after-effect of which is memory. Afterward, if *X* is aroused, the excitation passes over to *Y* because of the previous connection. And if *Y* is aroused, it passes over to *X* for the same reason. Usually both *X* and *Y* will have other connections established. The direction which an excitation will take from either at any given time will depend upon the relative strengths of the various connections.

Our hypothesis accounts only for connections established between simultaneous activities, but connections are also established between successive activities. How can we account for the latter type of connection? Neural activity does not cease suddenly. Suppose *X* is followed immediately by *Y*, and that *Y* is peripherally excited before the *X* activity has ceased; the *X* activity will pass into *Y* because *Y* will be most open. *Y* will not always go to *X*, but will take the path of least resistance. If another excitation, *Z*, is immediately peripherally aroused, *Y* will find *Z* most open and will discharge into *Z*. In a series of brain activities, each immediately and peripherally aroused, the strongest associative connections will be in the forward direction. The connections will not only be between adjacent temporal elements but between elements which are temporally distant from each other. *X* not only discharges into *Y* but to some extent into *Z*, provided *Z*-excitation starts before *X* has ceased. Direct connections are established between activity *X* and all other brain activities that come after, before *X* has ceased.

One of the characteristics of the good learner mentioned above is the ability to see significance. To the good learner significant meanings come quickly; or, at any rate, they *come*. To the slow learner they come slowly or not at all. The explanation, in terms of brain physiology is as follows: The meaning of any particular experience depends upon the organization of past experience. The organization of experience depends upon the associative paths established in the brain. The strength and persistence of these paths, as explained above, depended

originally upon focalized brain activity. Therefore, at any particular moment, the meaningfulness of an experience depends upon the degree of focalized brain activity at that time. The ability of an excitation to arouse activity in associative brain paths depends upon the pressure of the excitation. How pervious the associative paths are depends upon the strength of previous brain activities, for it was through such activities that the bonds or paths were organized. It is, of course, possible that that there are individual differences in the number of association fibers, the good learners having more of such fibers, which would give them a better associative apparatus.

The essential part of our hypothesis is, therefore, to be found in the focalized activity of the brain. The associative bonds, the forming of which constitutes learning, are established in focalized brain activities. There go on in the brain constantly facilitating and inhibiting processes. The best learners are those whose brains function best with reference to facilitation and inhibition. The brain of the good learner is selective. It is most serviceable and economical in the use of neural energy. In it there is always a sharp and well-marked separation of the focalized activities, and suppression of all other activities. On the other hand, the brain of the poor learner is not a serviceable brain. Its energies are wasted among many processes. It does not seem able to accomplish the segregation and facilitation of a few activities and the suppression of all others. The focalizing, facilitating, and inhibiting functions of the brain doubtless underlie general intelligence and general learning capacity. These characteristics of the brain which our hypothesis demands are probably innate and not much affected by experience.

ATTESTING PSYCHOLOGISTS FOR PUBLIC SERVICE[1]

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Some six years ago, there crystallized among certain members of the American Psychological Association, a sentiment towards the 'certification' of psychologists engaged in applications of the science. The chief source of this sentiment probably lay in the misuse of psychometric methods. It was felt that by officially certifying persons who had reached a suitable level of competence, the Association could diminish the risks arising out of irresponsible ultilization of psychology in the management of human lives.

As early as 1916, a group of members of the American Psychological Association had formed a society known as the Association of Clinical Psychologists. 'Clinical Psychology' is the designation seemingly decreed by usage for these applications of psychology concerned with conduct disorder. Shortly afterwards, in 1919, this group affiliated with the American Psychological Association, thus forming the 'Section of Clinical Psychology.' It was through the efforts of persons belonging to this group that the Association subsequently adopted a program of certification; and the nature of the program adopted was governed by the desires of this group.

The earliest document in the writer's hands related to the certification program is the report of a committee appointed at the meeting of 1918, and known by the name of its chairman as the Haggerty Committee. This committee took a broad view of its responsibilities, and in 1919 made a report, which was concerned with the mechanism of certification, and specifications for psychological examiners in terms of training

[1] Based on remarks at the meeting of the American Psychological Association at Madison, Wisconsin, December 27, 1923.

and experience. Its principles now govern. According to these recommendations there was appointed the existing Committee on Certification of Consulting Psychologists. This committee, under the chairmanship of Dr. B. T. Baldwin, offered a definite program in 1920. Chiefly, it appears, because the Association was thought not sufficiently safeguarded against certain legal complications, it was not immediately put into action. Further study seemed to have overcome this difficulty at the 1921 meeting, which authorized the issue to persons of relatively senior qualifications, of certificates attesting membership in a Section of Consulting Psychologists.

'All right, we'll take it, if you'll carry it,' are words often heard concerning some piece of camp equipment. In like manner, the Association as a whole has been willing to approve a certification program much desired by a small group, provided that group undertook to carry the program's administrative and economic burdens. This comes about because the Association includes a great many members whose interest in applied psychology is indirect. Under present developments in psychology this proportion may diminish, but is likely to be an important one for all time that can be foreseen. This large proportion of the Association's membership whose sympathetic interest is essentially that called for by courtesy, is by nature not an element of strength in the control of applied standards. In this respect more favorable conditions might have arisen if the Clinical Section had remained the independent body that it originally was, restricted to persons with definite major interests in that field. The special disadvantage of this would have been the separation of Clinical Psychology from its natural affiliations with the educational and industrial applications of the science.

'Raising the standards' in one's field of work is a captivating phrase to which most of us surrender at some time. But the raising of standards is not by definition a procedure in the public interest. Recent developments in the field of nursing have raised the standards of that occupation as such, but are generally thought to have lowered the quality of nursing

service available to the public at large. What standards are most in the public interest depends on how the service rendered fits into other more established technologies.

It was the dominant sentiment among those interested, that a consulting psychologist was properly a person of the doctoral level, of similar background and standing to the medical graduate. In practical consequence, the certifying action of the Association has been restricted to persons of the doctoral level with considerable additional experience whose material interest would be little, if at all, affected by the certifying action of the Association. Yet among 73 commentators on the matter whose opinions were sought by the Haggerty Committee, a clear majority of opinions expressed favored more than one level of standards. The issues involved are partly practical and partly, *sit venia verbo*, idealistic. If but one level were to be certified, the greatest public service would probably be performed by certifying a level approximating that of the New York State Association of Consulting Psychologists. The usefulness and vitality of this organization speaks forcibly for the practical wisdom of the standards it observes. The larger proportion of applied psychological work is of the psychometric type, not necessitating or practically involving a doctoral level of formal training. The work of this junior level is also more objectively defined and appraisable than that of the level now embodied in the Section of Consulting Psychologists. When an educational or clinical organization casts about for 'a psychologist' it is not persons of the now certified level that they are looking for, who are generally sought by name, but some one of less broad psychological training. It is at this level that the greater confusion reigns. The appointing authorities may in such cases be quite without the knowledge necessary to make well judged appointments. These were considerations that led the 1921 committee to suggest the attesting of a junior level, but it was not then acceptable. As noted, the senior level was accepted, and is embodied in the present Section of Consulting Psychologists. Certification by membership in a definite body was for legalistic reasons. The use

of the word 'Section' to denote it, was a blunder because of the confusion created with the Clinical Section and other prospective sections not denoting certification.

The scientific standards of this Section of Consulting Psychologists continue based on those laid down by the Haggerty Committee. No formal education suffices, because the things there required cannot be had without field experience. The average attainment now represented in it is above the average attainment of the just graduated M. D., which is no doubt gratifying to psychological *amour propre*, but of limited significance from the standpoint of public service. Under existing social conditions, the bulk of psychological service is to be rendered by persons of junior level in psychological science, and it is in regulating their qualifications that the greater public service will be done.

The Haggerty Committee formulated a quantum of education for the consulting psychologist. As noted, a person with such education would not fill its other requirements, but it represents the extent to which education as such can go. The doctoral degrees were not, *eo nomine*, insisted on. The optimum junior level probably means first-class formal training to the academic level of the M.A. (but not necessarily involving this degree), including field experience of about a year. It is an assurance of competence in fundamental techniques, but makes no claims in the field of synthesis, as does the present certificate for the Section of Consulting Psychologists.

Implicit in this is certification of persons outside the Association's membership, according to present requirements. The Association has raised its membership requirements not a little above what they were on the admission of several not unsuccessful protagonists of applied psychology. A definite sentiment has arisen that the present membership requirements are making the organization 'top-heavy.' An important class of persons, doing useful work in applied psychology, are excluded from public recognition of technological status.

It may be argued that this 'top-heaviness' exists wholly from the standpoint of applied psychology. Its constitution

defines the object of the Association to be the advancement of psychology as a science, and confines eligibility to those who are engaged in this work. From the standpoint of research, the advantage of a highly selected group of investigators need not be disputed. It is, however, a narrow view that science is advanced only by research in it. As in the case of medicine, profession and research advance both the science and each other. The course of events has made the American Psychological Association the natural representative of scientific psychology in all its social relations. This rôle cannot be sustained under an exclusive interpretation of these constitutional provisions. A national society with these functions, best serves them by bringing within its influence the greatest number of those who utilize the instruments of scientific psychology, either professionally or in research.

Membership in the Association has been by invitation, not by application. Such a mechanism is not very compatible with the regulation of public standards. This implies that any person upon such consideration as the Association may fix, may call upon the Association to evaluate his qualifications, and set or withhold the seal of its approval. Not until this is done, is a serious responsibility assumed concerning the regulation of public standards.

In 1921, the Clinical Section consisted by definition of persons whom the Association might certify at a senior level. But it was ineffective in the direct control of standards, because its membership has been by invitation. This was the chief *rationale* of the Clinical Section's desire for a special certification agency. The Certification Committee has had a number of quite logical applications which it could not consider, because certification has been since 1922 restricted to members of an itself highly selected Clinical Section. And if the Clinical Section had insufficient knowledge of a person desiring membership, neither has it had an effective remedy.

If therefore, as under the 1923 report, certification at a senior level shall be open only to members of accredited sections, the effective functioning of this certification depends on not too exclusive membership standards in the sections

participating in certification. The Clinical Section has been rather a selected body, paralleling in standards, though not in function or administration, the present certification program. Under these conditions it and prospective sections in applied fields will best serve the purpose of certification by including those members of the American Psychological Association who engage in the respective fields, independently of qualifications other than those for membership in the American Psychological Association. Practically, under such conditions any member of the Association is authorized to apply for the senior certificate, but he must do so through a Section authorized to participate in the certification program, as the Clinical Section is now. The proper body to determine his fitness is a selected group of leaders in the Section under which he applies. These decentralized Sections can provide the best mechanism for determining if the applicant has the qualifications for which a centralized Committee on Certification can best be responsible. Upon proper endorsement, the Certification Committee issues the required documents.

The question will again arise of distinct forms of certification for different fields. This has not as such entered prominently into committee reports. It has, however, been discussed at some length, especially at the Committee meeting in New York of August 5, 1920. Specialized certificates of the junior level seem not unreasonable, but at the senior, now recognized level, it has been deemed a doubtful procedure.

The essential question is whether the practitioner shall decide for himself what is proper for him to undertake, or have it decided for him elsewhere. In general the former is customary in the professions, the latter in the trades and crafts. There are relatively precise limits to what a carpenter, a painter, a marble-setter, a plumber, a steam-fitter may do, whose overstepping may have awkward consequences. On the other hand, there is little but professional discretion to stop a corporation lawyer from undertaking a divorce case, or an ophthalmologist from undertaking a laparotomy. In these fields the individual expert assumes responsibility for deciding his aptitude for the problems before him. There is a

story of a physician called to attend someone with the whole upper half of the body in pain, who declined on the ground that he was a specialist for the other half alone. But psychology is a science of the organism as a whole; and shall with difficulty cut herself even into three pieces without embarrassment and devitalization.

The attainment of the most respected status by the Association in this sphere implies some attention also to those standards of conduct generally understood as ethical. To such an end the Association is called upon to recognize standards of professional conduct, and be ready to remove as well as to exclude from its fellowship those who do not conform to them. For the nature of such standards, the experiences of the legal and medical professions furnish serviceable guides. The heartburnings and possible risks which such procedures entail are included in the cost of an influential profession.

It would seem that any person having at heart the influence of the Association in matters connected with public service, must desire in the first instance some means by which all engaged in the scientific applications of psychology may establish an official contact with the society. The Association is then placed in a position where it can offer an attest of competence in techniques, as well as at the more synthetic level, to any person qualified therefor. The determination of standards to be met is now properly centralized in the Committee on Certification of Consulting Psychologists. The determination of whether standards are met in an individual case still rests with this Committee, but is more properly decentralized, as contemplated in the Committee's 1923 report. At the level of psychometric techniques, it is believed that documentary attests should recite limited fields of competence, as clinical, educational, industrial; but at the synthetic, now existing level, this is not the case, for reasons above set forth.

In no other scientific discipline of comparable application to the affairs of men, are standards now so little controlled, and its name so irresponsibly utilized, as in our own. There exists but one natural authority to resolve this situation into

the one that normally prevails in scientific disciplines of recognized public service. That authority is the national body devoted to the furthering of our science, the American Psychological Association. Intrinsic obligation for this society to concern itself with standards in applied psychology is not asserted. The society has no more responsibility in this matter than it feels. But the principle has actually been accepted and the Association has begun the task of setting standards of applied psychology in public service. Few can regret this step, since the avoidance of concern for public standards must be ultimately paid for in terms of diminishing external prestige and esteem on the part of a not inactive portion of its membership.

DISCUSSION

A FURTHER OBSERVATION ON CATTLE AND EXCITEMENT FROM BLOOD

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In the September issue of the PSYCHOLOGICAL REVIEW Professor Stratton published[1] an account of an experiment upon cattle and excitement from blood. The experiment put to test the belief, almost unanimously held by cattlemen, that blood is a powerful emotional excitant to cattle. The result of the experiment cast doubt upon this belief; for the cattle upon which the experiment was tried showed but the mildest degree of excitement and fear, and, with one questionable exception, no anger at all.

There seemed a possibility, however, that, as he says, the 'cattle responded only as do those of the tamer sort, and that out upon the range the reaction to blood is characteristically different.' Accordingly Professor Stratton suggested that the work be repeated with range cattle of greater wildness; and, acting upon his suggestion, and with his advice, the experiment here reported was done.

The cattle experimented upon were a herd of approximately sixty. Of these twenty were cows, and the others, steers and heifers varying in ages from six to eighteen months. Twelve of the cows and thirty of the steers and heifers had run at large all their lives on ranges miles in extent. They were fairly wild, that is, they could not be closely approached by a person on foot. They were, however, not vicious. Eight of the cows and perhaps twelve of the steers and heifers had been raised, or had lived part of their lives on a farm, and were tamer than the others. These cattle were of mixed strains, with a predominance of Durham blood. The experiment was also tried on a three year old Hereford bull, running at the time with three old cows, in a small enclosed field.

In the experiment the work of Professor Stratton was repeated, using cow's blood which had been drawn and defibrinated two hours before. In addition fresh cow's meat was used as a stimulus in order to check the possibility that the excitement ascribed by cattlemen to blood was actually caused by the smell of flesh. For in most of the examples described by cattlemen there had been

[1] PSYCHOL. REV., 1923, 30, 380–387.

present, along with the blood, the bodies of dead animals, or open wounds.

The stimuli were presented as follows:

1. *With Regard to the Herd of Sixty Cattle on the Range*—A steady wind was blowing from the northwest; and so, to prevent the mingling of the odors from the different stimuli, the experimenter—who was on horseback—approached the herd at right angles to the course of the wind, that is, from the southwest.

When within two hundred yards of the herd he placed on the ground a clean section of a piece of burlap that had been torn into three sections. This was regarded as a control. The experimenter then moved a hundred yards further from the cattle, and, still at right angles to the wind, laid down another section of the burlap to which was fastened, face up, a clean strip of toweling, saturated with blood and covered with fibrin. A hundred yards further in line with the other two sections, the third section of burlap, which contained several pounds of meat, was placed. And finally a hundred yards on, blood was poured over the dry grass, covering a spot about three feet in diameter.

The cattle were then manœuvred in a crescent-shaped course toward the stimuli so that they approached each stimulus from the leeward, and so that they were reached by the odor of only one stimulus at a time. The order of presentation was, (1) clean burlap, (2) bloody burlap, (3) meat in the burlap, (4) blood strewn on the grass.

2. *With Regard to the Three-year-old Hereford Bull*—The clean burlap, then the bloody burlap, and finally the meat enclosed in the burlap, were placed one at a time in the bull's pasture, and he was driven toward them.

The results here are in agreement with the results obtained by Professor Stratton. In none of the situations was there an action that could be interpreted as a display of anger: there was no horning the ground, no bellowing, no fighting among the cattle. There were no signs of fear save the rather cautious approach and a slight start here and there. In every case, however, attention was given to the stimulus; the animals drew close to it, smelled it, turned away and then came back, fixated it with an apprehensive look.

To give examples, when the cattle were driven toward the clean burlap they walked within thirty yards or so and then slackened their pace and stepped gingerly, as if on thin ice, until they were

within twelve feet. There they stopped and stood motionless while one cow and then another walked forward, put her nose on the burlap, looked at it for several seconds, and then walked on past it. After that most of the others walked up to it, two or three at a time. Within five or six minutes the herd seemed to have lost all interest in it.

Much the same sort of behavior was shown by the herd when driven up to the bloody burlap, and to the burlap containing the meat. In these cases the animals remained longer sniffing the burlap, and more often came back to sniff again. Also, there were more sudden starts by members of the herd, and too, the interest of the herd lasted longer. Interest was shown for ten minutes. There was no apparent difference between the behavior shown toward the bloody burlap and that shown toward the meat in the burlap.

The herd was driven at a trot toward the blood strewn on the dry grass. When the leaders were about thirty feet away, the entire herd stopped abruptly and then edged around, forming a circle. One cow walked forward and smelled the blood for longer than a minute, the others looking on. Gradually her tail straightened out horizontally. She walked on, boxing some of the younger stock aside with her horns as she passed. Others walked up and sniffed. The tail of one other cow and of one steer straightened out. The herd did not lose interest in the blood for fifteen minutes. There was no evidence that the wild cattle reacted in a different way from the tamer ones.

The responses of the Hereford bull were not noticeably different from the responses of the range cattle. He walked up slowly to each of the three stimuli, and showed but little interest in either the clean burlap or the bloody burlap, merely sniffing for a minute. He paid more attention to the meat in the burlap, smelling it for two minutes with forelegs spread wide apart. Then he walked away three or four steps, gave a short, quick jump, walked back and smelled for thirty seconds longer, and then walked away.

This experiment seems to substantiate the conclusion suggested by Professor Stratton, namely, that blood—and we may add cow's flesh—does not call forth intense emotion as its simple native response in cattle; but that the excitement and anger mentioned by cattlemen is a response to a complex stimulus, in which, added to the blood, there are cries of the animal, sight of the body, and other similar elements which derive their effect from the herd experience.